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PLATES.

PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

EIGHTY-EIGHTH SESSION.

PRESIDENT'S ADDRESS.

I.—*On some of the Problems of Modern Physiology.* By JOHN G. M'KENDRICK, M.D., LL.D., F.R.SS.L. & E., Professor of Physiology in the University of Glasgow, and President of the Society.

[Read before the Society, 5th November, 1890.]

IT is just ten years since I began to take an active interest in the affairs of this Society. At that time Allen Thomson, Andrew Fergus, Ebenezer Watson, William Wallace, Joseph J. Coleman, and Henry Muirhead were members of Council. These and many other members of the Society have passed away, and this place knows them no more. It is only a few weeks since we lost Dr. Henry Muirhead. An adequate notice of him will be read to us this evening, and I therefore forbear saying more than that by his removal our Society has lost a true friend, and Glasgow has lost a man of character who had high aims and noble purposes. But, although individuals disappear, the Society has a corporate life, and its work will go on while it carries out its important functions of aiding in the diffusion of knowledge, of encouraging research, and of serving as a centre in Glasgow for all who are interested in the advancement of science.

Last year you did me the honour of electing me to the office of President, and now, at the beginning of a new session, it is my duty to deliver an address. Various topics presented themselves to my mind as worthy of your attention, such as a general review of scientific progress during the year, the impending changes in our University system, or the present position of technical education in Glasgow; but, on the whole, you will probably be better pleased if I address you on my own subject, and lay before you some reflections on the Problems of Modern Physiology. It is not my intention to weary you with such details as may be found in memoirs, and in the pages of text-books, but rather to approach the subject from the philosophical side, and to indicate the present position of physiological thought and speculation with regard to some of the more profound questions that occupy the attention of physiologists.

In tracing the history of any science one soon discovers that there are times when, in the wake of a great discovery, or of a wide generalisation, it is necessary to recast the whole subject, or to view it from a new standpoint. The introduction of the atomic theory into chemistry, the recognition of the law of conservation of energy in physics, and the adoption of a theory of evolution in biology, are examples of such epoch-making events. Again, a science occasionally makes a new start. She leaves the old and well-gleaned fields and explores new territories, adding to her stores at every footstep. Such happened in chemistry when the investigator first went into the domain of organic compounds, in physics when, as we have seen even in our own day, the field of electricity has been traversed by a thousand workers, and in biology when the naturalist turned his attention to the study of development or embryology. A similar change has happened in physiology; at all events, so many new facts have been collected in recent years, by the experimental mode of inquiry, as to make it necessary to review the whole ground, and to recast some of our older notions.

Physiology is intimately connected with anatomy, and most people would say that anatomy was the older science. This is true in one sense, but not in another, as it is evident that men in bygone ages must first have watched the movements of animals, the taking of food, and other physiological processes before they began to dissect. They took the body of the animal to pieces to see how it worked. Thus anatomy arose, and its study is justly

regarded as preparatory to the understanding of all vital actions. In later times anatomists have devoted more attention to form and structure, apart from a consideration of the processes by which these have been evolved. This department of science is now called morphology. It has its own problems in determining the outward appearance, internal build, and structural relations of organised bodies.

Modern physiology deals with the actions of living beings on their environment, and with the action of the environment on living beings; and it considers structure only so far as it illustrates or explains action. The progress of the science has shown, however, that there are numerous problems on which a knowledge of structure throws no light. Thus the movements of the body are effected by muscles. We study anatomically the framework of bones, the joints, the arrangement of the tendons, and their attachment to the bones; and, by a study of these mechanical arrangements, we obtain an idea of how any particular movement is accomplished. But the fact that the movement is effected by a contraction of the muscular fibres by which their ends are approximated remains unexplained, and we are obliged to ask the question, Why do the muscles contract? We find that muscular matter will contract on direct irritation, and that the stimulus usually causing it to contract is a change passing to the muscle along the nerves. This leads us to investigate the actions of nerves, and we ascertain that the change coming along a nerve to a muscle, which causes it to contract, originates in a brain or spinal cord, or nerve centre, as it is termed; further, we find that the change in the nerve centre may in turn be excited by influences coming to it from the periphery of the body along sensory nerves, and that these influences may, by acting on the nerve centre, originate muscular movements at once, or after a long delay. Then, again, we arrive at a knowledge of the fact that many of the changes in these nerve centres are connected with consciousness; and, finally, we must conceive a nerve centre as frequently in a state of molecular agitation or change—currents flowing in or passing out, connecting the sensory surfaces with the muscular and other apparatus, and welding the whole body into one harmoniously-working machine. These are all phenomena beyond the range of the anatomical mode of inquiry.

In the next place, we find that food and air are necessary to a

living being. The body of a living being is composed of complicated and unstable chemical substances, capable of being oxidised, and of being reduced by oxidation to simpler and more stable compounds, with a setting-free of energy. These more stable compounds are water, carbonic acid, nitrogenous compounds such as urea, and salts. Substances similar in character are produced from organic matter whether it be quickly oxidised by burning, or more slowly in the animal body, or still more slowly in the changes that occur when a body is allowed to decay in the earth. During life, simple compounds are being constantly produced by oxidations. At every moment some part of the substance of the body is suffering decay by undergoing combustion, and complex substances full of latent energy are by processes of oxidation reduced to simpler substances containing only a little energy or none at all. Thus we arrive at the astonishing conclusion that "every act of life is the outcome of an act of death."* The contraction of a muscle, a beat of the heart, every action of the nervous system, the formation of something in the cell of a gland—all these processes are connected with the splitting up of complex into simpler molecules, or, in other words, with the crumbling down, in a molecular sense, of the tissue. In the case of muscular tissue most of the energy thus liberated is set free, either as heat, or as motion in the form of muscular work. Thus energy is continually expended, and to make up for the loss we take food, which is a mixture of highly complex energy-stored bodies, and we breathe in oxygen whereby the oxidisable matters are reduced to simpler compounds, and the kinetic energy set free does work. But the food is not all oxidised simply in passing through the body; some of the compounds formed from it become, for a time, part of the living matter itself.

Thus we see that the ultimate problems of the physiology of the present day are concerned with the changes in living matter on which the phenomena of life depend. This is the characteristic of the science as it at present stands. The first physiologists attempted to explain the phenomena of the body by assuming the existence of animal and vital spirits. The animal spirits were more closely connected with the phenomena of animal life, whilst the vital spirits were common both to animals and plants. By-

* Michael Foster. Art. "Physiology" in *Ency. Britann.*

and-by it was found that many phenomena could be accounted for without such agencies, and a more mechanical explanation was adopted. The next step was the recognition of the uses of organs of the body, and the body was conceived to be built up of a series of machines, all more or less independent, but all co-operating in the general life. The work done by an organ was termed its function. The value of this line of thought was so great that it soon dominated physiology, and for a long time it reigned supreme. Further, we must not forget that this view of organs and functions produced brilliant results in physiology. Thus the doctrines of the circulation of the blood established by Harvey are the outcome of a study of the mechanism of the heart and blood vessels, considered as organs; and many other problems, such as the movements of respiration, the mechanism of voice, &c., are capable of solution by the application of mechanical principles to anatomical facts. The anatomical mode of inquiry, however, as we have already seen, did not solve every question. When applied to the case of the liver it showed only the arrangement by which blood was carried to and from that organ, and the ducts for carrying away the bile. The function of the liver was therefore said to be to secrete bile, and for many years this was considered to be the only function of that important gland. Another example of the failure of the anatomical method alone is that for many years no function whatever could be assigned to certain organs, such as the spleen, a large and complicated structure, existing apparently without a purpose, but now known to be concerned in the elaboration of blood and, possibly, in the disintegration of effete materials.

The next great conception, which was the completement of the doctrine of organs and functions, was the cell theory. This defined the cell as the physiological unit, and all functions of an organ were said to be the aggregate of the functions of the individual cells forming that organ. It was hoped that by a study of the forms, sizes, and general properties of cells, a complete knowledge of function would be obtained.

The application of the cell theory as an explanation of physiological phenomena received a shock by the discovery, about the middle of this century, by Claude Bernard, of the glycogenic function of the liver. He showed that the secretion of bile was not the only function of that organ, which, in addition, formed a large quantity of starchy matter, to which the name of glycogen was given. This discovery could not have been made by the most

diligent scrutiny of the cells of the liver with the highest microscopic powers, and a mere knowledge of the arrangements of the gland as regards blood vessels, ducts, and cells would have left this function unknown. On the other hand, the discovery might have been made by one having no knowledge of the anatomical structure of the liver. Bernard also demonstrated that this animal starch played an important part in the nutrition of tissue, more especially of the muscles.

As science advanced, the definition of a cell laid down by the founders of the cell theory had to be modified. The original definition of a cell was a small body having a cell wall, cell contents, and a nucleus or kernel, and it was assumed that here we had the fundamental structure with which the phenomena of life were associated. But discovery showed that these phenomena might be manifested by a mass of matter destitute both of cell wall and of nucleus, and consisting only of jelly-like matter, containing a few granules. Then arose the theory of protoplasm. The matter called protoplasm was now regarded as the material basis of life, and the great step was taken of abandoning all conceptions of life as the outcome of organisation, or the mechanical result of structural conditions. This view of physiological processes regards all vital phenomena as products of certain essential endowments or fundamental properties manifested in living matter. Organs merely modify the activities of living matter, and the function of an organ is the expression of the activities of the living matter composing it. The arena of life was shifted from the cell to protoplasm.*

Research, however, soon showed that there are various kinds of protoplasm, and that even protoplasm, when examined by special methods, affords indications of structure. It is not a homogeneous jelly, nor a granular jelly, as has been described, but it often contains a fibrillar network, in the meshes of which there is some kind of matter. It is not, however, strictly correct to speak of this fibrillated arrangement as a permanent structure, because it is not constant. It varies much from time to time even in the same mass of protoplasm. This protoplasmic matter, as found in an amœba or in white blood corpuscles, manifests the phenomena of life: it moves; it assimilates dead food, and raises it to the level of living matter; it is the seat of chemical changes of a very

* See author's paper on "The Modern Cell Theory and the Phenomena of Fecundation." *Proc.* Phil. Soc. of Glasgow, vol. xix., p. 71.

active character, and, in particular, it has very great reducing powers. It has been shown that the protoplasm of vegetable cells reduces salts of silver, and, founding on this chemical phenomenon, Loew and Bokorny have advanced the theory that living protoplasm contains a chemical compound of the nature of an aldehyde.* Its characteristic condition is one of incessant change; molecular processes are constantly going on in it, leading to the up-building or the down-falling of its substance. Whether protoplasm be a single highly complex chemical compound, or a number of such compounds, we do not know; but even if we suppose it to be one compound, it probably does not exist for more than a very brief space of time in the same condition. At one instant it is made, and in the next it is unmade. It is built up of matter that was once dead but now is alive. Dead food rises by a series of steps to the condition of living matter; this living matter is protoplasm; then the descent on the other side begins, and ends in the matter being split up into the simple substances which are what we may term the waste products. The dead food, itself only partially stable in character, becomes still less stable as it rises into the complex living material, and the instability arises from chemical affinities being unsatisfied. When it reaches the level of living matter it is so unstable that the chemical compound represented cannot exist, and on the slightest stimulus it explodes, in a molecular sense, and then begins its downward career. The downward course is a series of explosions, by means of which the energy latent in the living protoplasm becomes kinetic. Some of this liberated energy is used up within the material itself, so as to repeat the process of converting dead food-stuff into living matter; and the rest of the energy leaves the body chiefly as heat or motion. These processes are now expressed by the terms (first employed by Dr. W. Gaskell), anabolism and katabolism, the former denoting the up-building and the latter the down-pulling process, or, in other words, anabolism is winding up, and katabolism is running down. Both of these processes work in response to external stimuli. Thus certain nervous stimuli are anabolic, while others are katabolic—the first causing the living matter to rest and the second rousing it to action.

The intrinsic properties of protoplasm then are (1) *assimilation,*

* Oscar Loew and Bokorny: "Die Chemische Kraftquelle in Lebenden Protoplasma." Munich, 1882. See also author's "Text-Book of Physiology," vol. i., p. 60. Glasgow, 1888.

or the power of converting dead food into its living self; (2) *movement*, or change of form,—*contractility*, as it is usually called,—arising from internal disruptive changes; and (3) *irritability*, or the property of responding to stimulation by changes so minute as to escape the observation of the unaided senses. To state the facts in another way, the functions of protoplasm are the absorption of oxygen; the discharge of water, nitrogen compounds, such as urea, and carbonic acid; the production of heat, light, and electricity; and the performance of mechanical work. Chemical processes are related to all these forms of activity, or, in other words, we may say these properties of protoplasm are fundamentally chemical. But these characteristics are manifested in different degrees by different specimens of protoplasm, and even by the same protoplasm at different periods of its existence, for the changes occurring in one kind of protoplasm may be such that the decompositions result in energy being set free only in the form of heat, no visible change of form ensuing. This is the case with the protoplasm of the nervous tissues and secreting cells. Again, in another specimen of protoplasm, such as exists in the muscular and other contractile tissues, the energy may be set free both as motion and as heat, and the most striking result of the molecular change is movement. It is the business of the physiology of the future to determine what it is that causes one kind of protoplasm to differ from another. If it depends on internal structure, that must be of a molecular character, which no scrutiny with the highest microscopical power can ever detect, as the optical method of inquiry has probably come near its limits. It is true that, as the protoplasm manifests more and more one or other of its fundamental properties, structural characters may gradually make their appearance; but even these seem to be of subsidiary importance, and not to be the real cause of the specific kind of action. For instance, contractile tissue may, by long-repeated activity and specialisation of function, take on the structural characters of striated muscular fibre, but these characters are not essential to the contraction of the fibre. Many fibres contract that are destitute of the structural characteristics of striated muscle. Nor is even the form of a fibre necessary to contraction. The fibre contracts because it contains protoplasm having the intrinsic property of altering its form, owing to molecular changes excited by a stimulus. In this way we gain the position now reached by the most profound thinkers in physiology, that while protoplasm

exists in differentiated varieties, each variety still retains more or less of the general properties of protoplasm. In other words, while there is variety in the results of protoplasmic activity, there are underlying in all kinds of protoplasm certain fundamental properties of a primitive substance.

I might illustrate this by tracing the differentiation of the protoplasm of the cells forming the primitive layers of the embryo, showing how the various tissues are built up, and from these the mechanisms which we call organs, and pointing out especially that the functions of all these organs depend on the activities of the protoplasm contained in them. Hitherto the attention of physiologists has been largely occupied, and very properly occupied, with investigation into the general mechanism of these organs. For example, we are now acquainted with the arrangements of the circulation of the blood, viewing the heart as a contractile force-pump driving the blood through a series of elastic and contractile tubes, the blood stream being guided by valvular structures. There is probably not much to be done in this direction; but there still remains the problem of the molecular changes happening in the wall of the heart itself. What is the cause of the heart's rhythm? What relation has the rhythm to the activity of the nerve cells in the wall of the heart itself? Again, we know that the blood is driven through the tissues and organs, and that each element of living tissue selects from it the matters which it needs for its growth and activity, but there are hidden molecular processes in the structures separating the blood from the tissues and concerned in the absorption by the tissues of the nutrient materials. We have been too much in the habit of regarding some of these phenomena as simply physical processes like endosmose and exosmose, forgetting that the structures through which the nutrient materials pass are composed of living protoplasm, which so modifies the physical process as to make it unlike what would happen if the membrane were dead.

Are we getting any insight into molecular processes concerned in life? This question may be answered by studying the changes in the cells of a secreting gland. Take, for example, the pancreas. Its microscopic structure shows a number of cells grouped around a space at the end of a canal, which is the beginning of one of the ducts of the gland. Each cell is a minute spheroidal body, composed of protoplasm, having a nucleus, and showing a network of fine fibres. We may disregard the nucleus, as it has nothing to do

with the secretive activity of the cell, and is concerned only in its reproductive processes. One side of the cells is bathed in a fluid called lymph, which has transuded from the capillaries in the neighbourhood; the other side is free and projects into the space. Secretion consists in the accumulation of fluid in this space, and as it is formed it is carried away by the ducts of the gland. Now, few of the elements of the secretion, if any, exist preformed in the blood; they must, then, be produced by the activity of the cells of the gland. The cells are nourished by the lymph, and they grow and undergo various changes that have been observed only during the last few years. It has been noticed that in the cells there are periods of rest alternating with periods of activity. During the period of rest the cell grows and increases in size, and this is owing, not so much to increase in the network of the protoplasm, as to an accumulation of material in the meshes of the network. It has further been noticed that as the material in the meshes increases the network diminishes, pointing to a conversion of the substance of the network into the matter in the meshes. Sometimes this matter may be seen as little granules, and the granules collect chiefly at the side of the cell next the lumen of the canal. During active secretion these granules disappear, and the cell diminishes in bulk, and the living protoplasm gets rid of its stored-up material. The cell may still be growing, but the growth of protoplasm is chiefly during the period of rest. But a further discovery has been made. It has been ascertained that the ferment-matter in the pancreatic juice—a substance called tripsin—does not exist in the cells of the gland. The particles in the cells I spoke of are not particles of tripsin, but are particles of a material that readily yields tripsin. This material has been called tripsinogen, or generator of tripsin. The order of events would seem to be (1) the growth of protoplasm from materials supplied by lymph; (2) the conversion of portions of this protoplasm into tripsinogen; (3) the ejection of the tripsinogen; and (4) its conversion, probably in the act of ejection, into tripsin. We thus get a glimpse into the processes going on in this wonderful little laboratory. Possibly we see only several of the steps of the process. From the matter in the lymph to the protoplasm of the cell there may be many transient stages, and again from the fully-formed protoplasm to the particles of tripsinogen there may be another series of steps. It has also been suggested that the protoplasm by one magical stroke converts the

material of the lymph into living matter, and then, in an instant, there may be the degradation of a portion of the protoplasm into tripsinogen. The evidence points rather in the direction that I have indicated.

Another illustration of molecular action is found in the study of the changes in muscle when it contracts. Here we have, not secretion, but a change of form—contraction. The evidence is gradually accumulating in favour of the view that this contraction is due to a sudden change (of the nature of a decomposition) of a contractile matter in the protoplasm of the muscle cell. When a muscle contracts there is the evolution of carbonic acid, the muscle becomes acid, from the formation of a variety of lactic acid, and there is evidence of other chemical changes. Do these substances directly come from the protoplasm of the muscle cell? There are facts pointing to a striking analogy with the changes in the secreting cell—namely, that the protoplasm of the muscle cell forms a contractile matter, and that this contractile matter is torn to pieces when a muscle contracts. The products of the decomposition are ejected; the protoplasm reconstructs the contractile substance at the expense of its own material; and the muscle cell is again ready for another contraction. The energy is set free both as motion and as heat, while in the case of the secreting cell it appears as heat only.

There is every reason to believe that molecular changes of a similar kind occur in the nervous system. We know next to nothing of the chemical changes happening in nervous tissue, but a study of the nature of the nervous discharges that stimulate muscular fibre to contract, and a review of what is known of the order of events in various kinds of reflex actions, all favour the view that these phenomena depend on molecular disturbances of a chemico-physical kind. When a nerve is irritated, a change or molecular disturbance is propagated along it to the muscle; this change acts on the matter forming the nerve endings; and the changes in these, in turn, set up molecular ruptures in the contractile stuff of the muscle cell Again, when a sensory impulse reaches a nerve centre, it may stop there—that is to say, its energy does not seem to be sufficient to do more than set up changes in the substance of the cell; but in other cases, or at other times, the change in the nerve cell may be propagated to other groups of nerve cells, and these, in turn, may originate impulses causing muscular contractions. The fact that time is spent in these

processes, the phenomena of fatigue, the formation of waste products, and the necessity of repair, all point to the occurrence of molecular changes.

One of the most remarkable characteristics of living matter, when it is adapted to the manifestation of energy in a particular form, is the economy with which the manifestation is accomplished. Thus in the case of muscle, energy is liberated as heat and motion, and the manifestation of these modes of energy is the result or expression of the chemical processes occurring in the muscle. A muscle may be regarded as an instrument or machine primarily for the production of motion, and the heat liberated is, in a sense, of secondary importance. Considered as a machine, it is a striking fact that as much as one-fourth of the energy of a muscle may appear as work, the remaining three-fourths as heat. The small amount of matter undergoing change is also remarkable. A frog's muscle, weighing less than a gramme, may be caused to lift twenty times its own weight through a height of five millimetres, at intervals of two seconds, for at least an hour without showing much fatigue, and the finest balance would scarcely show any loss of material. Taking man's body as a whole, observation has shown that about 25 per cent. of the total energy appears as mechanical work, about twice that of the best constructed steam or gas engine, which yields from 12 to 15 per cent. as work, the remaining 88 or 85 per cent. of heat being lost.*

Another instance is the economical production of electricity by electric fishes. The Torpedo (*Torpedo galvani*) has about 1,000,000 plates in its two electric organs; the electro-motive force of a single plate is ·0000117 of a volt; and the total E.M.F. of the organs at rest, if all the plates were simultaneously called into action (which probably never occurs), would thus be 11·7 volts.† At the moment of discharge it is much increased, as indicated by the strength of the shock, but I have been unable to find exact measurements. The electric organ of the more powerful electric eel (*Gymnotus electricus*) contains something like 8,000 plates, each of which has an E.M.F. of ·00006 of a volt.‡ The total E.M.F. of the organ at rest is

* See "Text-Book of Physiology," *op. cit.*, vol. ii., p. 445.

† E. du Bois-Reymond: "Living Torpedoes in Berlin." In "Biological Memoirs." Edited by J. Burdon-Sanderson, p. 442, Oxford, 1887.

‡ Du Bois-Reymond, *op. cit.*, p. 442. Also Carl Sachs' "Untersuchungen am Zitteraal," p. 278, and p. 300.

therefore about ·48 volt, but at the moment of discharge it may rise as high as from 300 volts to 600 volts. The animal will give shocks of this strength at short intervals during several hours without showing fatigue; and this enormous expenditure of energy is obtained with a very small amount of tissue change. Only a very small portion of the energy is set free from the electric organ as heat—a portion so small as to have hitherto escaped detection. In August last, in experimenting at Stonehaven with the electric organ of the common skate *(Raia batis)* with the use of delicate thermopiles and a sensitive galvanometer, I was able to detect an evolution of heat in the electric organ on stimulating the nerve supplying it.

It will probably be found that in the organs of the electric fishes we have a production of electricity on an economical basis far surpassing anything yet contrived by man.

Still another example of the economy of nature in physiological processes is furnished by a remarkable series of observations recently made at the Alleghany Observatory, by Langley and Very, on the production of light by the Cuban fire-fly (*Pyrophorus noctilucus*, Linn.).* By the use of a bolometer, these observers were able to make photometric measurements of great delicacy and accuracy, and they have shown, to quote their own words, "that the insect spectrum is lacking in the rays of red luminosity and presumably in the infra-red rays, usually of relatively great heat; or, that it seems probable that we have here *light without heat*, other than that heat which the luminosity itself comprises, and which is but another name for the same energy" (p. 273). Experimenting with an amount of heat which would, in the time of one operation, only raise the temperature of an ordinary mercurial thermometer by less than one four-hundred-thousandth of a degree centigrade, they found that there was practically no heat in the spectrum; or, to put it in another form, "it follows that the insect light is accompanied by approximately one four-hundredth part of the heat which is ordinarily associated with the radiation of flames of the luminous quality of those which were the subject of experiment. . . . We repeat that nature produces this cheapest light at about one four-hundredth part of the cost of the energy which is expended in the candle flame, and at but

* See *London, Edinburgh, and Dublin Philosophical Magazine* for September, 1890, p. 260.

an insignificant fraction of the cost of the electric light or the most economic light that has yet been devised; and that, finally, there seems to be no reason why we are forbidden to hope that we may yet discover a method (since such a one certainly exists and is in use on a small scale) of obtaining an enormously greater result than we now do from our present ordinary means for producing light."*

Living matter, then, exists in different conditions. The protoplasm of a muscle cell is not the same as that of a secreting cell, and both differ from the protoplasm of a nerve cell. Again, the protoplasm of a contractile cell from a sheep probably differs from that of a man, and there may be varieties even in individuals of the same species. And yet in all the kinds of protoplasm we have, in a subsidiary way, some of the fundamental properties. Thus all kinds of protoplasm assimilate and are irritable. By living protoplasm, then, we mean that state of matter which does not ascend higher in the scale of molecular complexity, but breaks down on stimulation into simpler compounds. It is matter in a state of incessant change. When it dies it ceases to be protoplasm. According to this view, protoplasm exists only a brief space of time. The materials which we get from dead muscle—albumen, sugar, fat, saline matters, water—were never alive as such; there is no such thing as living albumen; these are matters formed on the death of the protoplasm, or, as we have seen, they have been derived from complex bodies that have once been protoplasm possibly existing in the meshes of the network at the time of the death of the protoplasm.

It is interesting here to notice, even at the risk of a little repetition, some of the attempts that have been recently made by philosophical biologists to gain an insight into the constitution of living matter.† At one time it was supposed that the oxygen absorbed in respiration in a sense purified the blood by oxidising effete matters in it. It was shown, however, by Pflüger, in 1872, that the oxygen is not merely for the removal of effete matter, but that it serves as food for the protoplasm. This view directed

* The luminous surface of the fire-fly gave ·0024 calories per square centimetre per minute, or only as much heat was radiated by the insect as would raise a mercurial barometer between two and three millionths of a degree centigrade.

† See Address to the Biological Section of the British Association, by Professor Burdon-Sanderson, F.R.S. *B.A. Report*, 1889.

attention to the protoplasm as the arena in which oxidations took place, and inquiries were made as to its constitution. It was soon recognised, in the first instance by the botanical physiologists, that protoplasm consists of two portions, one forming a kind of framework, and the other a more fluid part, existing in the meshes of the framework. The framework, according to this view, is stable and is living, and the more fluid part is not alive, but it is in a condition of active chemical change. The living part forming the framework acts on the non-living part after the manner of a ferment, exciting changes in it without itself taking part in the chemical operations thus induced. It has thus, to use the term of Berzelius, a catalytic action, and living matter, as we see it, may be regarded as consisting of a catalytic substance, the framework, and of a catalyte, the semi-fluid material. All oxidations occur in the catalyte within the meshes of the living framework. You will observe that this modifies the popular conception that the structure of the body is not permanent, but is undergoing renewal and decay. The more modern view suggests that there are parts of the framework of our tissues that may remain intact for long periods of time, and that it is only a portion of the tissues that undergoes tear and wear and renewal.

Further, Nägeli has suggested that living matter is composed of particles called micellæ, far too minute to be seen with the highest microscopic powers, and, in fact, identical with the molecules of the physicist. This conception has been modified by Pfeffer, who recognised that a micella, the ultimate element of living matter, according to this view, is not a molecule but a group of molecules, probably of different kinds. To this he gave the name of tagma (τό τάγμα—that which has been ordered or arranged; a division, a brigade) as indicating a system or group. By the union of tagmata the framework is formed, and in its meshes, according to Sachs, water may be condensed to less than its normal volume, as in the vital turgescence of plants. Life does not therefore occur in slime, but in a framework, some conception of which may be formed by the idea of a sponge.

Animal physiologists have not advanced so far in this direction. The only attempt has been made by Engelmann, who has applied the terminology of Nägeli and Pfeffer to the structure of muscle. The structure of striated muscular fibre resembles that of a crystal, inasmuch as each portion resembles every other portion. If we

cut a muscle transversely, a number of cylindrical parts are exposed, and each of these may be regarded as made up of a large number of still more minute portions, termed ino-tagmata by Engelmann. Each minute ino-tagma is a short cylinder, which during life has its axis parallel with that of its neighbours, and during activity there is a flux or flow of the catalytic matter from each pole to the equator. When a muscle fibre contracts, according to this view, the change does not occur in the tagmata but in the matter between them, and it consists in a flow of matter from poles to equator, the flow occurring along with sudden oxidations, change of electrical state, and disengagement of heat. Bernstein* has applied this theory to the explanation also of the electrical phenomena of muscle. The result is that we may regard the chemical operations, electrical phenomena, evolution of heat, and change of form as manifestations of the same process.

What conception can we form of the nature of life, after a consideration of these statements? Some thinkers have attempted to draw a distinction between vitality, as the special property of certain collocations of matter, and that peculiarity of living beings by which they possess unity and individuality. Vitality, they say, is inherent in each cell, but an organism exists as a whole by the reciprocal action of its elementary parts. The question is asked—Has this interdependence of parts its efficient cause in the properties inherent in living matter itself, or does there reside in the body a power which adjusts those parts into conformity with an idea, arranging and combining them for a definite purpose? My own opinion is that we cannot draw this distinction between the life of the body as a whole and the life of its individual parts. The somatic life is the expression of the lives of the millions of living particles of which the body is composed. Whether, however, we contemplate the general life or the life of any particular cell, is our explanation to be teleological or physical? Is there any intelligent agency at work or are the phenomena of life merely the outcome of the laws of molecular physics? A teleological explana- is the only one in my judgment that meets the case. Adaptation is characteristic not only of every organism as a whole and as

* For details, see Burdon-Sanderson's Address. *Op. cit.*, p. 8. Also Bernstein: "Neue Theorie der Erregungsvorgänge und Electrischen. Erscheinungen an der Nerven- und Muskel-Fasern." Untersuchungen aus dem Physiologischen Institut, Halle, 1888.

regards its environment, but also of the parts of the organism as regards their mutual relations. Adaptation in the body or in the individual cell cannot be explained by the operation of physical laws alone, working according to the rules of blind necessity, and a careful study of the phenomena of life suggests something outside the mechanism with which we find it associated. But what is the nature of this external agent that works upon and co-operates with matter to produce the phenomena of life? It is not satisfactory merely to give it a name and call it vitality or the vital principle. This explains nothing. As we cannot think of an agent or force acting independently of matter, may not some of the properties of living matter depend on matter in peculiar conditions with which we are as yet unacquainted?

Few, if any, physiologists would now-a-days assert the existence of a vital principle. A special vital force is not assumed as an agent in explaining physiological problems in simple structures. The term may be used for convenience, serving, in the language of Kant, as a resting-place where "reason can repose on the pillow of obscure qualities;" but it explains nothing. Nor is it enough to say that vitality is a property of protoplasm. Is it not nearer the truth to regard it as a special condition of matter? Physicists consider the properties of things as the result of movements either of what may be termed the grosser particles of matter, or of an ether which has a substantial existence; and there are theories in physics by which the constitution of even matter itself is referred to properties of the ether. May we not also get a conception of the vital state when we consider it as a mode of motion? The properties of living protoplasm may be regarded as the result of intricate molecular movements. It is not necessary, however, to restrict these movements to the molecules of matter that explain to the physicist certain physical phenomena, such as cohesion, elasticity, chemical affinity, but to those more subtle and evanescent movements which he assumes to occur in an ether, and by which he attempts to explain the properties of light and electricity. No doubt physicists are acquainted with facts which practically prove the existence of this ether; and, by assuming that it has certain properties, they can explain the phenomena of light, and many of those of electricity. I readily admit that no such attempt has yet been made in the sphere of physiology, but it is not to be lost sight of. At the same time, it is evident that, even if we were able to account for

certain vital properties by a reference to movements in the ether, we would still be confronted by the same problem. At one time men thought they had got to the secret of life when they reached the cell; now they desire to come to closer quarters; but even supposing they worked out the molecular movements in, say, a nerve cell in the cerebrum, they would be no nearer an explanation of consciousness, with which the vital activities of a nerve cell are undoubtedly associated. All that I plead for is that, when we attempt to refer the phenomena of life to movements of matter, considering matter in the widest sense of the word known to physicists, physiologists are not to be charged with gross materialism, more especially when they assert that even these ethereal movements will not account for all vital phenomena, more especially those of consciousness.

Many dislike such speculations because they have a gross conception of matter and of its properties. This dislike would vanish if they took a more profound view of the constitution of matter, and if they gave weight to one of the lessons of physiology—namely, that our knowledge is limited by the nature of our organs of sense. We have no right to assume that we are acquainted with all the properties of matter; and it is not unreasonable to suppose that the phenomena of vitality are in their essence due to movements which cannot be detected by the most delicate and refined instruments, but which may some day be reasoned about on a basis as sound as the undulatory theory of light. Arrest the movements by the removal of water, as in dried seeds and dried infusoria, and the phenomena of vitality cease until they are again moistened. Stop the movements by cold, and the organism becomes dormant; but the molecular machinery starts again when it is raised to a certain temperature. Heat the living matter beyond a given temperature, and the molecular movements are so altered, disturbed, retarded, or quickened, as to be incompatible with the living state, and the machinery flies to pieces. The properties of different kinds of living matter depend, according to this view, not merely on the properties of the chemical elements or chemical compounds which we can extract from it after it is dead, nor on the play of ordinary physical forces, nor on the action and reaction between the living matter and its environment, but on the particular kinds of molecular motion occurring in the living stuff. Thus vitality may be regarded as a dynamical state. It is a condition of matter *sui*

generis. It may be as vain to speculate on the origin of vitality as it is to inquire how certain movements of the primitive atoms first started certain physical phenomena. We may be content with the belief that the Creator of the Universe called matter into existence, endowed it with properties, and started its atoms into those mazy dances of molecular motion on which the phenomena of vitality depend.

It is significant to note the change that has taken place during the last few years in the physiological mind regarding the true nature of physiological processes. Fifty years ago, Mayer of Heilbronn, in a celebrated treatise, showed the relation between organic motion and the exchange of material in living organisms, and he demonstrated that certain functions, regarded as vital, could be measured, and the results referred to standards of quantity. He pointed out quantitative relations between different kinds of energy, and did much to lay the foundation of the present theories held by physicists regarding energy. This work exercised an immense influence on physiological discovery. It guided Von Helmholtz in his researches into the physiology of nerve and muscle, Du Bois-Reymond in his investigations into the electrical phenomena of living beings, and Ludwig in his demonstration of the hydrodynamics of the circulation. The general effect of such researches was to curtail apparently the sphere of vital processes and to lead to the acceptance of physical explanations of physiological phenomena.*

For many years the majority of the processes occurring in the body were supposed to be explicable by chemical and physical laws, and too little attention was paid to the circumstance that these processes did not occur in dead but in living matter. A strong reaction against an exclusive mechanical view has undoubtedly set in. It is now recognised that while the mere movements of the blood in the heart and vessels are subject to the laws of hydrodynamics and hydrostatics, the activity of the heart itself, the contractility of the smaller arteries, and even the special kind of elasticity seen in the coats of the larger arteries, cannot be explained by physical laws. Again, while absorption from the stomach and intestinal canal of the food-stuffs prepared in the processes of digestion is in accordance so far with the physical laws of exosmose and endosmose, we must not forget that the absorptive membrane is covered with millions of living cells, each of which apparently

* See Burdon-Sanderson's Address. *Op. cit.*

exercises a selective power over the substances presented to it, appropriating certain constituents and rejecting others. Another example is the secretion of urine—in some respects a physical process of filtration of certain matters from the blood, but a process modified by living cells that line the capsules of Bowman and the uriniferous tubules. Much was expected from the discoveries of the electrical phenomena of muscle and nerve, but it must be confessed that these phenomena have not thrown much light on the real nature of muscular or nervous action. The purely physical phenomena have been worked out with surprising success. Thus we are acquainted with the mechanism of the eye considered as an optical apparatus, and we find the eye behaves according to the laws of refraction, but we know little yet as to the specific action of light on the living structures of the retina. We can apply the laws of acoustics to the arrangements in the external and middle ear for the conduction of sound, but the mechanism of the inner ear by which these vibrations are communicated to the living cells, and by these transformed into nervous impulses passing to the brain along the filaments of the auditory nerve, is still shrouded in mystery. Our explanations always fail when they are applied to the phenomena of living matter. Hence, a new school of vitalists is forming among physiologists, and a conviction is gaining ground that our present methods for the investigation of the mystery of vitality, even when manifested by the simplest cell, have for the present reached their limit. The further we push our researches the more complex the problems become. The notion that the first phenomena of life are manifested by a jelly-like structureless matter, as we have seen, must be abandoned. Careful investigation with the highest microscopic powers, and with the best staining reagents, shows details of structure hitherto unnoticed; but a discovery of this complexity of structure only reveals a new set of problems, and does not help us in knowing how the living thing manifests its wonderful properties.

Nor can a study of the chemical processes occurring in living things help us much in approaching the secret of vitality. To take an example, I may adduce what is now known as to the formation and absorption of peptones in the stomach. Not long since the explanation of the formation and absorption of peptones appeared to be that they were dialysable bodies formed from proteids by the action of the gastric juice, so that such proteid

matter might readily enter the blood. They are formed by a process of hydration, a molecule of water becoming united to a molecule of albumen. This view is confirmed by the fact that if we act upon peptone with a substance having the power of removing a molecule of water from it, such as anhydrous acetic acid, the peptone may be reconverted into albumen. But a more careful study shows that the process is not so simple as was at one time supposed. In the first place, there are only the faintest traces of peptones in the blood, and, in the second, their presence in the blood in any appreciable quantity is followed by poisonous symptoms. Large quantities of peptone are poisonous. It would thus appear that the living cells covering the mucous surface of the stomach have the power of taking up these peptones, and that after changing them into serum albumen by a dehydration, they pass on the serum albumen to the blood stream. Here we have an instance again of the peculiar activity of living cells. Peptones readily dialyse through a dead animal membrane, such as the thin wall of a stomach, but when the wall of the stomach is alive they are converted into serum albumen. Why, then, should peptones be formed in the first instance? The only answer that can be suggested is that proteid matter in the form of peptone is the appropriate food for these living cells, that these cells pick peptones out of the digested matters bathing their surface, and then pass them on into the blood, transforming them in the act of passage into serum albumen. Many other instances might be given, all tending to show that there are peculiar chemical activities in living matter, and that we are very far yet from accurate knowledge on this subject.

Still there is no ground for despondency. Physiologists must push their researches by physical and chemical methods into the arcana of living matter, in the hope of getting information as to the molecular machinery on which vitality depends. It is a first condition of success that we clearly define the limits beyond which chemical and physical explanations cannot at present pass. We know then where we are, and from that point we can go on and ask more searching questions. Science recognises no impassable boundaries; and although the difficulties in the way of a final answer to many physiological problems are enormous, we may expect that as years, perhaps centuries, roll on, these difficulties will gradually disappear, and the darkness will be dispelled by the steady light of more profound knowledge.

II. — *Memoir of Henry Muirhead, M.D., LL.D.*
By Henry Dyer, M.A., D.Sc., C.E.

[Read before the Society, 5th November, 1890.]

Henry Muirhead, M.D., LL.D., formerly President of this Society, was born at Pollokshaws, on the 4th of March, 1814. His father, James Muirhead, was a tanner and skinner, a business with which some of his relatives are still identified in this city. He obtained his elementary education at the public school of his native town, and on leaving it he was apprenticed to Pickering Brothers, curriers, "Wee Doo' Hill," Glasgow. During his apprenticeship he attended evening classes, chiefly of a scientific nature, although he did not neglect such opportunities as occurred for improving his general knowledge, a favourite practice of his being reading in staircases, by the help of stair gas, while he was out with messages, a practice which, although it might lead to his own improvement, was not likely to be encouraged by his employers. These incidents are mentioned for the purpose of showing that he was largely self-taught, and explaining some of the traits in his character. Like almost all self-taught men, however distinguished they may have become in after life, he showed a certain want of a systematic training in those subjects which afterwards engaged his attention, the possession of which would have enabled him to have taken them up in a more logical manner. This deficiency, however, may have been more than counterbalanced by the greater originality of his methods and the force of his character, for too often what is called education "only makes a straight-cut ditch of a free meandering brook," and enthrals the mind with mere words or methods. Dr. Muirhead, very early in his career came to the conclusion that the end of education was to train to right reason and independent judgment, to moderation of mind and to virtue, and his life was a proof of the success of his personal efforts in training both head and heart. While still an apprentice, and pursuing knowledge under difficulties, he displayed great interest

in the family of his widowed sister, teaching the children to read, and to a great extent taking the place of a father to them.

After his apprenticeship he attended the classes at the University of Glasgow, necessary to qualify for a degree in medicine, his father assisting him to pay the fees. He obtained the degree of Doctor of Medicine in 1844, and about the same time was admitted a Licentiate of the Faculty of Physicians and Surgeons, the Fellowship of which was bestowed upon him in 1855. During his career as a medical student, and also after its completion, he was connected with the Royal Infirmary and Fever Hospital, the latter at that time being in close proximity to the former. While in the Infirmary he was seriously ill with fever, having caught infection at the Fever Hospital. In connection with that fact may be noted a point of some importance in the history of the practice of medicine. In a letter to the *Glasgow Herald* of 10th January, 1883, Dr. Muirhead "protested against a statement which had appeared to the effect that it was only thirty years or so since Sir William Jenner showed clearly that typhoid fever was an entirely different ailment from typhus fever," and stated that the fact was, that fifty years ago, ten years before William Jenner was admitted to the medical profession, Dr. Robert Perry, of this city (and father of the present Dr. Robert), first demonstrated the distinctiveness of these two diseases. He stated that Dr. Perry did this for years amidst much opposition and obloquy, which had not ceased when he became his clinical clerk, in 1841. He concluded his letter by the rather characteristic remark "that the spreading of this delusion by a portion of the Faculty in London is but part and parcel of a scheme these gentlemen have been long and earnestly working at, namely, to filch from us the education of our medical students, which has been for generations one of the glories of Scotland."

On leaving the Infirmary, Dr. Muirhead purchased a medical practice in Hutcheson Street, and afterwards removed to Dundas Street, Glasgow. He, however, only remained in practice a few years, and by and by became assistant to Dr. Hutchison, the Superintendent of the Royal Asylum at Gartnavel, and thus was led to the special study of insanity. During his assistantship at Gartnavel he married, and shortly afterwards purchased the house and grounds of Longdales, near Bothwell. Here he built a private asylum, which, with the assistance of his wife, he con-

ducted with much success. As the result of hard and faithful work and much frugality, he was able to add to his possessions at Longdales, and in the course of years he found himself the owner of a considerable landed estate, and of other means, and was thus able to retire in 1868 from Longdales, and from the active practice of his profession. At the date mentioned he purchased the small estate of Bushyhill, Cambuslang, where he constantly resided till the time of his death, which occurred on the 31st of July, 1890.

There is not much in the events of such a life as I have briefly sketched to attract attention or general interest, but there was much in the man which was worthy of notice. A gentleman who saw a good deal of him during his latter years writes to me that to him "he seemed a painfully shy, silent, and retiring man—painfully, I say, because you could see his inner goodness of heart; and a genuine desire for free social intercourse was constantly fighting against an external reserve and unreadiness of manner, begotten by long years of retired and uneventful life." This, no doubt, was a common impression which he produced, for to strangers and even to those who did not know him intimately, he had a very distant manner; but among his friends—and the circle was a wide one—he was cheerful and genial, full of a certain humour and pathos, which, under favourable circumstances, very often found expression, through the medium of a friendly letter, in little outbursts of verse. Although noted for the frugality of his life, and even thought sometimes to be rather exacting, and "near" in his financial transactions, he seldom allowed any appeal for help which really deserved consideration to pass unnoticed, and his subscription list was lengthy, and often contained considerable sums. He felt, in short, that the practice of benevolence was a work to be gone about conscientiously, and not simply as an excuse for the non-performance of duties. In the village of Cambuslang he found many opportunities for displaying an interest in a cause he had very much at heart—namely, the social amelioration of the working-classes; and the Public Library and the Working-Men's Social Union, which flourish in that village, were among others, two schemes which always received his hearty patronage and assistance. In his will he left a free site and one hundred pounds sterling towards a permanent Public Library and Institute which it is proposed to erect in Cambuslang. His opinions on the social questions of the day verged in the direction of Socialism, and on

some points he was distinctly Socialistic. In politics he was, therefore, a strong Liberal, or, rather, Radical, for he delighted in the name of Radical. His opinions sometimes compelled him to become a leader in popular movements, especially such as were intended to preserve the rights of the working-classes. For "rights of way" he had almost a mania; and a few years ago his assertion of a right of way through the Borgie Glen, near Cambuslang (in defiance of the Road Trustees, and which might technically have been construed as an offence at law), led in the end to victory, and to the acquisition for the public of certain fields known locally as the Applebraes. But when such work was done, he would withdraw into the seclusion which he loved, and betake himself to his books, leaving it to any one who desired applause to claim the triumph.

His interest in education, however, extended beyond the village in which he lived. In December, 1876, he presented to the University of Glasgow the sum of £2,100, and in November, 1879, a further sum of £400, to endow a Demonstratorship of Physiology in connection with the Chair of the Institutes of Medicine—the main object being the promotion of medical science by the training of young men of suitable capacity to become teachers and investigators of physiology. In 1886 he received from the University the honorary degree of Doctor of Laws in recognition of his scientific and professional position, and of his munificence in founding the Muirhead Demonstratorship for the advancement of medical education. He was for a good many years one of the Trustees of Anderson's College, and took a great interest in its welfare. In a letter to his nephew, of 8th July, 1878, I find him saying, "I have been using my little influence of late in furthering a scheme for removing the medical classes of Anderson's College near to the University, and replacing the males of Anderson's College by women studying medicine. At present the attempt does not look hopeful, but patience and perseverance may do something." This extract shows that the medical education of women had occupied his attention for a considerable time. He adds, in the same letter, that "two years ago a lady applied to be admitted to the preliminary examination of the Faculty of Physicians and Surgeons. Only other two voted with myself to admit her. Last week another applied, and the application was only lost by a majority of two." On the organisation of the Glasgow and West of Scotland Technical College, he was appointed

by the Faculty of Physicians and Surgeons to be their representative on the governing body, and he attended its meetings with great regularity. He took a special interest in the work of the Bursaries Committee, of which he was a member. It is rather curious to note that in one of his letters, written in 1878, he states that "he knew old Allan Glen, who was a native of the 'Shaws, and made some verses on him forty years ago." Allan Glen's School is now an integral part of the Technical College, and fifty years after writing his verses on its founder, Dr. Muirhead shared in the responsibility of making the school which bears his name part of a much wider organisation for technical education.

Dr. Muirhead's connection with this society dates from 1866, and, after serving on the Council for some years, and also holding the office of Vice-President, he was President for three years from 1883. He represented the Society for a good many years at the annual meetings of the British Association, in which he took a great interest, and of which he presented faithful reports to this Society. His papers, which appear in our transactions, show great width of reading and observation, although in some cases the want of an early systematic course of study is clearly discernible. His favourite topics were those which connect physiology, physics, and psychology, and these form the subjects of his most important papers. His Presidential Address in 1884, and his paper on "The Senses," read before the Society in 1877, of which the address was practically a continuation, contain his views on many problems of great interest to physiologists, physicists, and psychologists, all of which were handled in a manner which showed that the author had thought them out for himself. In the discussion on the paper, our President remarked "that he had listened to it with great pleasure, as the author had certainly taken a very wide view of a subject which would lead into very intricate and difficult questions. He was particularly interested in the purely physical way in which Dr. Muirhead had tried to examine sensory impressions. This was the true way to get at a thorough understanding of nervous mechanism. By investigating physiological problems in the way indicated in the paper, physiologists might help such thinkers as Dr. Muirhead in still further effecting a complete correlation between physical forces and agencies outside of themselves, acting on the terminal organs of sense, and those processes which take place in our

nervous mechanism. Such investigations as those Dr. Muirhead had made were of great value, and this was especially true of those into the mechanism of sensory impressions and into the more obscure actions of the nervous system. Pondered over, and studied in all their bearings by thinkers, these investigations would assist in laying the foundation of a sound psychology."

In connection with these subjects Dr. Muirhead took considerable interest in psychical research, hypnotism, &c., and was for years a member of the Psychical Research Society. He was, moreover, a diligent student of sociology, and of its relations to the general theory of evolution. Although early a believer in this theory, he was not satisfied with the forms in which its principles were stated. "Survival of the fittest" he thought very inaccurate, and preferred to speak of the "elimination of the unfit and unfortunate, with survival of the fit (not fittest) *and* fortunate." For many years before his death he took regular meteorological observations at Cambuslang, which are recorded in the reports of the Scottish Meteorological Society. He took an active part in promoting the observatory at the top of Ben Nevis. From meteorology he extended his studies to astronomy, and in the last volume of our Transactions there is a paper by him in that department. Anthropology was perhaps the study which he most enjoyed, and for some years he was a member of the Council of the Anthropological Institute of Great Britain. He occasionally contributed papers on this subject to the British Association. He published many letters in *Nature* and other journals dealing with the topics which occupied his attention; and from the rather desultory record which he has left it may be said that almost all the subjects which are accessible to one who was neither a mathematician nor a practical linguist were tackled by him to a certain extent.

Although Dr. Muirhead could not be described as a traveller, he delighted in visiting places of interest and beauty in this country, and very often threw his observations into verse. He made two journeys to the Continent—the first, while still a young man, being *via* Rotterdam and Paris; the second in 1878, in the company of his brother and nephew, through some parts of Germany, Austria, and France, in which he paid special attention to places of historical and antiquarian interest. In 1884 he visited Canada, along with two nieces and other members of the British

Association. Illness prevented him from attending the sectional meetings, but he and his party prolonged their tour considerably, visiting San Francisco, Chicago, Washington, Philadelphia, and other places.

Reference has been made to his practice of throwing his thoughts into verse. This, indeed, seems to have been his favourite form of literary composition; and in addition to his published papers he leaves a great mass of manuscript verses, which include monographs of considerable length on various scientific and philosophical subjects. Many of these are without title. Some of the titles are "On Dreaming," "On Language," "Force, Energy, and Rhythm." In 1879 there was printed for private circulation, by some of his friends, and unknown to the author, a pamphlet containing some of his verses under the title of "Heretical Rhymes," in which he has a tilt at some of the current scientific, philosophical, and religious opinions. It must be admitted that in many cases his versification is somewhat rough, although some of his lyrical pieces are not devoid of sweetness as well as pathos. In a letter written shortly before his death he said that "this habit of writing verses has been a solace to my declining years, for as Burns says :—

> 'Crooning to a body's sel' does weel eneuch;'

whether it may be of any service in enlightening the world is another matter, though I had hoped it would." One of his pieces, on "Religion," is interesting, as giving his views on a matter which moulds a man's character, and supplies the springs of all his actions. After enumerating the chief creeds of different ages and nations he says :—

> "All can't be right—all may be wrong—
> Is the sad surging of my song.
> God grant me grace to have my hope
> On this apophthegm penned by Pope—
> 'For forms of faith let fools and zealots fight,
> He can't be wrong whose life is in the right.'"

This may be called his confession of faith. He was in the habit of attending Cambuslang Parish Church so long as his health permitted, but he looked upon religion not so much as a matter of creed as of life, and of earnest endeavour for the good of others. He felt deeply that property or possessions of any kind, either mental or material, had their duties as well as their

rights—in fact, that property had no rights apart from duties; and this feeling explains points in his character, which to a superficial observer appeared peculiar. Having no family of his own, he, at a very early part of his career, seems to have determined to devote the greater portion of his means to some public purpose, although the exact nature of that may not have been determined till about two years before his death.

Reference has already been made to the great help he obtained from his wife in the management of his asylum at Longdales. After her death, his niece, Miss Annie Muirhead Shearer, kept house for him until her death in 1887, and it was largely through her influence that he began to take a more active interest in local and other social matters. The cause of the equality of the sexes in matters connected with education, trades or professions, and even in politics, had his full sympathy. We have seen that in 1878 the medical education of women had engaged his attention. These circumstances seem to have influenced him in the disposal of his property—the greater part of which, amounting to about £30,000, he left to a body of Trustees for the purpose of founding and maintaining an institution for the instruction of women in physical and biological sciences (but not theological), to fit them to become medical practitioners, dentists, electricians, chemists, &c. The following extracts from a memorandum which accompanies his will may be quoted, as explaining the motives which influenced him, and illustrating some of the traits in his character. He says:—"On the consideration that I have all my life been very much indebted to the aid of women—to my mother, my wife, my eldest sister Jeannie, and her maiden daughters; and seeing how small a share of real, good, solid, and scientific education has been accorded to women, I have been induced to bequeath the greater portion of my savings for the purpose of erecting and partially endowing an institution or college, somewhat after the model of the old Andersonian, for the education of women *by women*, so far as that can practically and judiciously be carried out. I do not wish it to be called Victoria or Queen's (little has either done for poorer sisters); but since all the afore-mentioned women bore the name of Muirhead, I think it may be fitly named after them. I have not named any medical men as trustees, because (as yet) their trades-unionism is opposed to women entering the medical profession. I do not wish clergymen to have anything to do with the management of the college, for creeds are the firmest fetters to

intellectual progress; and a man who cannot break loose from such himself is not the best hand to help others." He makes certain suggestions to his trustees regarding the constitution of the college or institution, but he has left them great freedom in making their arrangements. It would be out of place even to speculate on the probable results of this bequest; but, as one of the trustees, I will say that our only object will be to carry out his intentions in such a manner as to lead to the greatest public good.

III.—*A Problem in Ventilation by Heat.* By W. P. BUCHAN, Sanitary Engineer.

[Read before the Society, 19th November, 1890.]

SOME time ago I was testing the speed of the air up the ventilating pipe from off the ceiling of a church. The vertical part of this pipe was about 40 feet high, while the diameter of the pipe was 18 inches.* Near to the bottom of the vertical pipe there was a small circular gas tube, with provision for lighting a dozen gas jets when wished. I first tested the ventilating pipe without the gas being lighted, when the upward speed indicated was 160 linear feet per minute. With the gas lighted the speed rose in five minutes to 300 linear feet per minute. This was no great speed, but it showed that the heat of the gas gave considerable increase of up-current — nearly double. Then the question occurred, Would the speed be still further increased by suspending a piece of 12-inch diameter pipe, and, say, 3 feet long, of thin sheet-iron, a little above the gas jets, so that when those jets were lighted they would heat this 12-inch tube, and so increase the up-current?

As it was going to be rather troublesome to make the experiment with the 18-inch pipe, I. constructed a 6-inch diameter pipe, 3 feet long, with a 3-inch diameter inlet at its foot, but to one side (as per C in diagram), the bottom being closed with a lid. The 3-inch diameter inlet was to suit the anemometer.

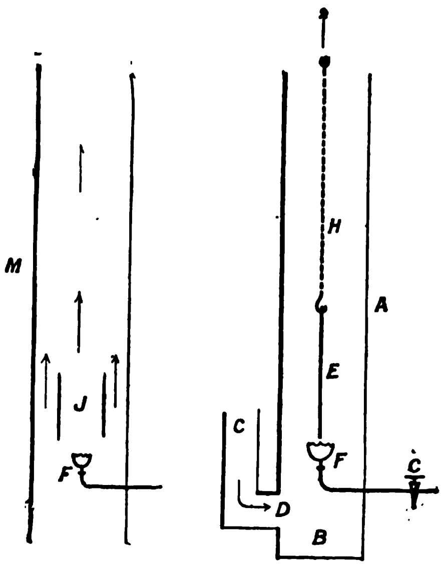
Upon suspending a piece of 3-inch diameter thin sheet-iron tubing, one foot long, above a No. 4 Bray's gas burner placed inside of the 6-inch pipe, the speed indicated, with the gas lighted, was 520 linear feet in two minutes. With the 3-inch tube removed the speed rose to 585 feet in the two minutes, showing a difference of 65 feet. The inner tube in this case, therefore, did more harm than good.

Another experiment was thereafter made with a piece of plain sheet-iron, about 1 foot long and 5½ inches wide, suspended a little above the gas (as per E in sketch), with the result that with this sheet-iron plate in, the speed indicated was 555 feet in

* The pipe would have answered its purpose better had it been 36 inches diameter.

two minutes. Upon removing the plate the speed rose to 580 feet in two minutes. The use of the plate would, therefore, appear to be a mistake and a pure waste of money also — the up-current being about 5 per cent. less with it than without it.

This loss of speed with the inner tube, or the plate, suspended above the gas I attribute to the extra friction.



In order to get the full value of the heat and conserve it for the up-current it would be all right to wrap asbestos or felt round the outside of the pipe A or M in sketch, but suspending a large concentric pipe or a plate inside the outlet ventilating pipe in the manner shown appears to me to be a pure waste of money.* They retard the up-current, and so harm the ventilation, whether the gas is lighted below them or not, and in many cases in practice the gas would not require to be lighted, as when there was wind or other natural cause to produce a good up-current. In this latter case the suspension of a pipe or plate inside of the outlet pipe would simply be a continuous check. In fact, in thousands of cases, were the outlet ventilating pipes put in large enough, and fitted up properly, with satisfactory provision against down-draught, no gas would be needed, as, for example, for one-flatted schools, and for churches and many halls, &c.

In concluding, I may remark that, so far as can be inferred from the foregoing data, if, to tickle the fancy of the public, some metallic addendum must be hung up about the pipe to raise the wind, I respectfully recommend that a highly venerated servant be not overlooked, and that, therefore, an old horse-shoe be used; but it must be suspended upon the *outside* of the pipe.

* The diagrams are taken from Buchan's new text-book on "Ventilation," No. 271 of Weale's Series.

IV.—*On the Gravimetric Composition of Water.* By W. DITTMAR, LL.D., F.R.S., F.R.S.E., and J. B. HENDERSON.

[Communicated to the Society, 17th December, 1890.]

PREFACE.

FROM his famous research on the composition of water (*Ann. Chim. Phys.*, Series 3, vol. viii., page 189 *et seq.*) Dumas concluded that, in accordance with Prout's law, the "equivalent" of hydrogen, meaning the ratio $H_2:O$, is equal to one-eighth exactly, although the exact mean of his 19 syntheses stood at 0·12515 (corresponding to H = 1·00120 for O = 16, or to O = 15·98 for H = 1). Feeling convinced that his result was liable to a small negative correction, although he had already allowed for the adventitious water formed from the oxygen of the air which had been present in the dilute sulphuric acid which he used for the evolution of his hydrogen, and seeing that his individual results oscillated between ·1247 and ·1256, he had no hesitation in rounding off his mean and adopting ·12500 as the net result of his work. Strictly speaking, he had perhaps no right to make even this slight arbitrary correction, but his choice was confirmed very shortly after the publication of his memoir by an independent research of Erdmann and Marchand,* which, if it proves anything, shows that Dumas' adopted value is probably nearer the truth than his uncorrected mean.

With these two investigations before them, all chemists agreed in admitting that, however it may stand with Prout's law, the ratio O : H happens to be equal to 16 exactly, and they continued doing so until Stas, in the course of his great research, came to determine the silver value of sal-ammoniac; and finding that the weight of sal-ammoniac, equivalent to Ag = 107·93, that is to say, his "NH_4Cl" exceeded his sum N + Cl by more than four units, gave it as his conviction that the true ratio O : H cannot be greater than 15·96, corresponding to H = 1·0024 for O = 16. But Stas' own result is H = 1·0075, and this number, if it has

* *Journal für Practische Chemie*, vol. xxvi., page 461 *et seq.* Abstract in *Ann. Chim. Phys.*, immediately after Dumas' memoir.

any right to vote, is as incompatible with 1·0024 as it is with 1·0000. But Stas' guess was confirmed subsequently by Clarke, and, after him, again by Lothar Meyer and Seubert, who, in their respective recalculations of atomic weights, both arrived at precisely the same value. It is probably owing to this circumstance that the number 15·96 has been so generally adopted by chemists, and found its way into all the handbooks. All chemists, of course, have not adopted it in the same sense. With many, no doubt, it is no more than the arithmetically correct registration of what they take to be the most probable value for a strictly speaking unknown constant, which, for anything they can know, may perhaps lie closer to, say, 16·05 than to 15·96; but not a few attach to it a higher significance, inasmuch as they take 15·96 to be the true value, and contrast it with the old 16 as with a mere approximation, just as one might contrast Berzelius' and Dulong's value, ·764 for C : O, with Dumas' and Stas' ·7500. Prominent amongst these chemists are Lothar Meyer and Seubert, who, in a memoir published by them in the "*Berichte der deutschen chemischen Gesellschaft*" in March, 1889, give expression to their conviction in the most emphatic manner possible by insisting that we must henceforth refer our atomic weights not to O = 16, but to O = 15·96, for "dieser Wert ist der am meisten verbürgte und desshalb allen übrigen für diese Grösse Betracht kommenden vorzuziehen." But where are the guarantees? A glance at Lothar Meyer and Seubert's book* affords an answer, inasmuch as it clearly shows that their conviction of the correctness of their number is based chiefly upon Dumas' and upon Erdmann and Marchand's quantitative syntheses of water, which both, in their hands, led to the identical value, 15·96 for O : H. But, unfortunately, their calculations rest on a false basis. Their mode of calculating Dumas' determinations is that, viewing his 19 syntheses as *quasi* one synthesis, they deduce the value O : H from a combination of the sum of Dumas' 19 oxygen-weights with the sum of his 19 uncorrected water-weights, which comes to pretty much the same thing as if they had calculated the constant from the mean of Dumas' "*equivalents bruts*," meaning the equivalents as they come out if we neglect the adventitious water produced by the dissolved oxygen in the sulphuric acid used for making the hydrogen. But Dumas, as we see from his memoir, effected the corresponding

* "Die Atomgewichte der Elemente, aus den Originalzahlen neu berechnet, von L. M. and S." 1883.

correction in each of the 19 cases; and, if we believe in Dumas' work at all, what right have we to discard his corrections on his own numbers? Clearly, none whatever; we must go by his "*equivalents corrigés*," and their mean is ·12515, corresponding to O : H = 15·98.

To pass to Erdmann and Marchand: they, as Lothar Meyer and Seubert inform us, made eight syntheses, which, if calculated as one synthesis, lead to the value 15·96. But, when we look at the original memoir, we find that E. and M. made two distinct sets of four experiments each. In the first set they weighed their oxide of copper and metallic copper in the ordinary way, and allowed for the displaced air by calculation; in the second they adopted Dumas' plan, and weighed both the oxide and the metal derived therefrom in the same evacuated tube. And in each of these four latter experiments (as also in one of the first series) they took care to free their hydrogen from every trace of atmospheric oxygen by making it pass through a red-hot tube full of metallic copper, and from it through a U-tube charged with fused caustic potash before it reached its destination. In other words, while Dumas corrects for his adventitious water by calculation, they avoid its formation. Their general mean for the weight of water containing one part of oxygen, that is to say, for $H_2O : O$, is 1·12520. But, obviously, their second series is far more reliable than their first, and its mean stands at 1·12492, corresponding to O : H = 16·010. And these four determinations, being obtained by a decided improvement upon Dumas' method, are worth as much as Dumas' 19; hence the net result of the two investigations taken conjointly is $O : H = \frac{1}{2}(15{\cdot}981 + 16{\cdot}010) = 15{\cdot}996$, or, practically, the exact number 16·000.

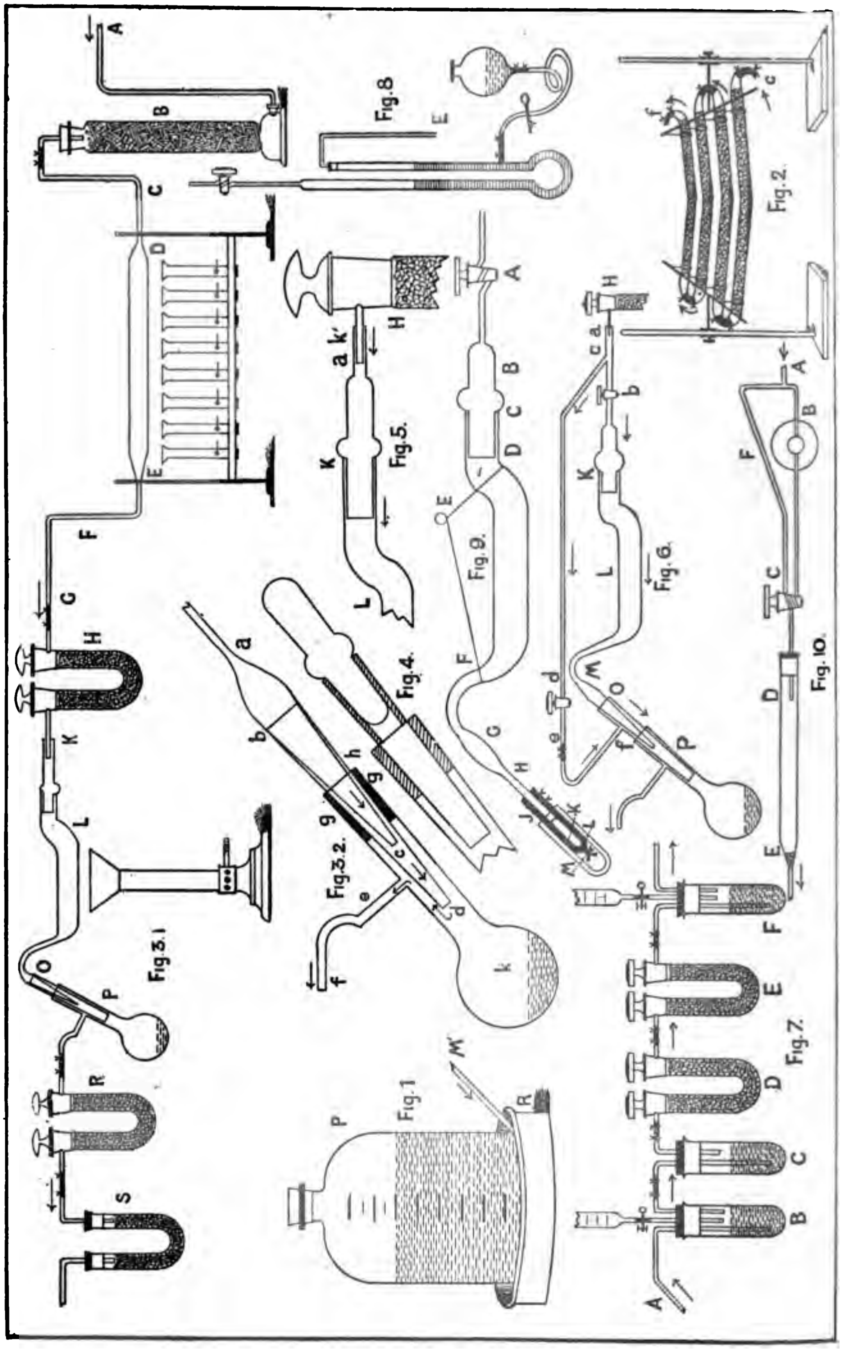
But, supposing even we had nothing but Dumas' uncorrected equivalent to go by, seeing that the 19 individual values for O : H, as calculated from these, vary from 15·89 to 16·02, it surely is worse than pedantry to swear to that 15·96 as if it were a revelation from heaven! So I thought, some months ago, when, on the basis of little more than a second-hand knowledge of the two researches, I came to speculate on the matter; and being, moreover, imbued with a strong mental bias in favour of Prout's law, I decided upon undertaking an experimental inquiry into Dumas' method-errors, feeling sure in my own mind that I should be able to prove that the old integer 16 is at least as good an approximation to the truth as the fashionable 15·96.

As a first step, I directed Mr. Henderson to set up as close an imitation as possible of Dumas' apparatus for the production of pure hydrogen, and then to determine the oxygen in what we soon came to call "Dumas hydrogen," by filtering a sufficient known volume of the gas through red-hot copper gauze, and weighing the water produced. The result more than confirmed my anticipation, but, while Henderson experimented, I studied Dumas' and Erdmann and Marchand's original memoirs, and made an unexpected discovery.

From Dumas' account it appears that, while he weighed his oxide of copper and oxide *in vacuo*, he weighed his water in the air, and that, consequently, his water-weights were liable to be reduced to the vacuum. That a man like Dumas should be capable of omitting this obviously very relevant correction would appear to be an absurd presumption, but the way in which he states his results (on page 200 of his memoir) tends to confirm it. On the table referred to he gives, in so many successive columns, (1) the weight of the water-receptacle before, (2) after the experiment, (3) the exact difference of the two weights as "the weight of water produced," (4) the value $H_2:O$ as calculated from the recorded weights of water and oxygen, and (5) the same corrected for the oxygen introduced as dissolved air with the dilute acid used for the evolution of the hydrogen, as the "*equivalent corrigé.*" Now, if Dumas puts us in a position to check his subtractions, then, assuming that the weighings (1) and (2), as they stand, are already reduced to the vacuum, he is surely bound to tell us how he calculated the respective corrections. But he does nothing of the kind. Assuming, as I did at the time, that he simply forgot the vacuum correction, all we can do now is to add to his mean equivalent of hydrogen the weight of air, displaced by 1·12515 parts of water. Assuming, as we may without fear of committing a serious error, that the mean density of the air in which he operated was the same as that corresponding to 15°C and 760 mm. pressure, we arrive at the value ·00138, which raises the equivalent of hydrogen to ·12653, corresponding to $O:H =$ 15·81! But, admitting this to be the real result of Dumas' determinations, how is it that Erdmann and Marchand, who *did* reduce their water-weights to the vacuum (there is no doubt about this, because they give us in each case both the uncorrected and the corrected number), arrived at the value 16·01? In going carefully over their report I, at last, came to see my way towards

a plausible explanation. As appears from their memoir, they dried their hydrogen, finally, by passing it through a U-tube, 3 feet long, charged with fused caustic potash, which, they assure us, dehydrated even a quick current of the gas completely, "because a U-tube filled with fused chloride of calcium, if appended to the outlet end of the potash tube, suffered no change of weight." Their water receptacle was combined with two chloride of calcium tubes to retain the moisture of the surplus hydrogen and of the air which was sent through the apparatus at the end of an experiment. They obviously considered it necessary to prove that fused caustic potash dries a gas completely, but they had no doubt about the efficiency of fused chloride of calcium. And this is no more than was at the time admitted by every chemist. But Fresenius (see his "Zeitschrift" for 1865, page 177) has since shown that a moist gas, after having been dried exhaustively by even fused chloride of calcium, gives up a quantity of water to oil of vitriol, which, in his trials, amounted to about 1 milligramme per litre of gas. Now, Erdmann and Marchand's test experiment with their potash-tube is quite compatible with the assumption that this tube retained every trace of moisture from the hydrogen which streamed through it, and it is not unfair to assume that the chloride of calcium which Erdmann and Marchand used *in their actual syntheses*, was about at a par with Fresenius' preparation. If I am right so far, it follows that Erdmann and Marchand lost about 1 milligramme of water for every litre of permanent gas which passed through their water-receptacle at the end of the experiment; and this, as a little reflection shows, is amply sufficient to account for all the difference between their value for $H_2O:O$ and Dumas' as corrected by me. But this, as I could not help seeing, would only prove that Erdmann and Marchand's determinations must be given up as hopelessly wrong, and, in any case, it still devolved upon me to prove the correctness of my suspicion concerning Dumas' calculations. Obviously, what I had to do was to inquire critically into both Erdmann and Marchand's and Dumas' methods, and besides to carry out at least a few syntheses of water according to Dumas' method, to enable myself to decide between his own and my mode of interpreting his numbers. And these "few test-syntheses" were sure to develop into an independent re-determination of the constant. After some hesitation, I decided upon carrying out this programme, and,

ILLUSTRATIONS OF APPARATUS EMPLOYED.



thanks to the indefatigability and youthful energy of my excellent collaborator, the work was completed in less time than I had thought it would demand.

W. D.

DUMAS' RESEARCH CRITICALLY CONSIDERED.

Dumas' method, or rather that of Berzelius (because it was he who invented it, as every student of chemistry is aware), consists in this, that a given quantity of oxide of copper is reduced in hydrogen gas, and the loss of weight suffered, that is to say, the oxygen given up, by the oxide, compared with the weight of water produced. This is no occasion for describing the exact *modus operandi* which was adopted by Berzelius, but we must name the refinements upon it which were introduced by Dumas. One of these was that he worked on a far larger scale than his predecessor had done, and another, that he took care to purify and dry his hydrogen more fully by passing it through long successive columns of (1) solution of nitrate of lead; (2) solution of nitrate of silver; and (3) pumice soaked in oil of vitriol, or powdered over with phosphoric anhydride. Dumas, besides, took special care to not allow the surplus hydrogen and the air which he passed through the water-receptacle at the end of an experiment to escape without having first been most thoroughly dehydrated by means of tared tubes containing one or the other of the two dehydrators named.

But the principal improvement by Dumas was that he evacuated his reduction-tube, both before and after the experiment, before taking it to the balance. The difference of the two weighings thus gave the *true* weight of the oxygen quite directly, and the errors, which would otherwise have been caused by the gases condensed in the pores of the oxide and of the metal, were eliminated at the same stroke. The water produced was weighed in the ordinary way—we mean "not *in vacuo.*" Referring to Dumas' memoir for more detailed information on his mode of working, we will now pass to a review of his results, as tabulated by him on page 200 of his memoir. A superficial examination of this table sufficed to show that it is not free from misprints. To detect these we re-calculated all his "*equivalents bruts,*" and, in doing so, had no difficulty in spotting and rectifying the errors.

The results of our calculations are included in the following table :—

Number.	n	k	$k - k_0$	*Order of Residuals.*
3	20	1·12481	- ·00066	19
19	31	1·12488	- ·00059	18
2	20	1·12490	- ·00057	17
8	46	1·12500	- ·00047	16
10	52	1·12504	- ·00043	15
1	13	1·12505	- ·00042	14
4	57	1·12506	- ·00041	13
16	37	1·12506	- ·00041	12
11	52	1·12512	- ·00035	10
12	60	1·12533	- ·00014	3
7	35	1·12546	- ·00001	1
15	56	1·12558	+ ·00011	2
5	76	1·12566	+ ·00019	4
6	44	1·12568	+ ·00021	5
17	34	1·12575	+ ·00028	6
13	62	1·12577	+ ·00030	7
18	32	1·12580	+ ·00033	8
14	52	1·12581	+ ·00034	9
9	60	1·12585	+ ·00041	11

Col. 1 gives the order in which the several experiments are enumerated on Dumas' table; col. 2, under "n," the weights of oxygen operated upon, rounded off to the nearest integer number of grammes; col. 3, for each experiment, the weight of water obtained per gramme of oxygen, as "k." In the heading to col. 4 the symbol k_0 stands for the most probable value of the ratio $H_2O : O$ calculated by us; the numbers in col. 5 arrange the values $k—k_0$ in the order of their magnitude, irrespective of their signs.

For any one of the 19 syntheses we have an equation of the form $W - Sk_0 = \delta$, where S stands for the (exact) weight of oxygen and W for the weight of water found, and δ for the error in the water-weight found, supposing k_0 to be the true value of $H_2O : O$. We brought the equation into the form $S(k - k_0) = \delta$ (which was more convenient to us, because we had already calculated the values k), and then, substituting for every S its corresponding n, calculated that value for k_0, which, supposing it to be substituted in all the equations, would reduce the sum of the squares of the values δ to its minimum. The result was $k_0 = 1·12547$. From the list of errors in the last column we calculate that the "probable" error of a single determination, is

$r = \pm \cdot 000303$, which, indeed, is not far removed from the tenth of the values $k - k_0$; for the probable error of the mean we find $r_0 = \pm \cdot 000070$. In all that we have stated the word "error," of course, means that part of the total error which is owing to accidental causes, and even in this sense r_0 does not represent more than a fraction of the uncertainty. For a guess the latter may be put down at three times the probable error; if we do so, our calculation shows that, supposing Dumas' determinations were free of method-errors, the true value of k could be said to lie between 1·1253 and 1·1257.

In the present case the accidental part of the error of any one result may be taken as being a function of the errors of the four weighings involved. Assuming, for an approximation, that the probable values of these four errors were all of the same magnitude $\pm x$, we have for the probable error in the water-weight W, and for that in the oxygen-weight, S, the same value, $x\sqrt{2}$, and for the relative probable errors in W, and in S or $\frac{1}{S}$, the expressions $\frac{x\sqrt{2}}{W_0}$ and $\frac{x\sqrt{2}}{S_0}$, respectively. Hence we may write—

$$W \times \frac{1}{S} = \left(W_0 \pm \frac{x\sqrt{2}}{W_0} W_0\right) \times \left(\frac{1}{S_0} + \frac{x\sqrt{2}}{S_0} \cdot \frac{1}{S_0}\right)$$

Hence, according to a rule of the method of the least squares, if r be the probable error of the product,

$$r^2 = \frac{W_0^2}{S_0^2} \cdot \frac{2x^2}{S_0^2} + \frac{1}{S_0^2} \cdot 2x^2$$

Hence, as $\frac{W}{S} = 1 \cdot 125$ very nearly,

$$r = \pm 2 \cdot 129 \frac{x}{S_0}.$$

Dumas' average value for S was about 44 grammes; hence we have for what one might call his average "experiment,"

$$x = \pm 6 \cdot 2 \text{ milligrammes.}$$

In other words, the value ·0003 recorded above for the probable error of a single experiment may be explained by assuming that the probable error of each of his four weighings was $= \pm 6$ mgm., and that the real error varied from something like -18 to something like $+18$ mgm.; and this is a fair enough guess, considering that the apparatus he used were uncommonly heavy and bulky. His reduction-tube, for instance, cannot have displaced much less than 600 mgm. of air, and, supposing the density of the air to

have changed between the two weighings by $\frac{1}{30}$ of its value, this alone would make his oxygen-weight wrong by $\pm$ 20 mgm. To pass to *Dumas' Method-Errors,* the most obvious of these is the one caused by the unavoidable presence of atmospheric air in even the most carefully prepared hydrogen gas. As already stated in the prefatory note, Dumas sought to eliminate this error by calculating for each of his syntheses the weight of water produced from the atmospheric oxygen introduced as part of the sulphuric acid used for the making of the hydrogen, and deducting the result from the total weight of water as found by direct weighing. His mean "*equivalent corrigé*" is by ·00018 less than his corrected mean. Deducting this from our $k = 1{\cdot}12547$, we have for the corrected number 1·12529, or, for O = 16, H = 1·00232. Unfortunately, however, Dumas does not tell us how he procured the data for his correction; and, besides, if we consider that his hydrogen had to travel through some seven metres of U-tubes, charged, some of them, with porous materials, before it reached its destination, it is impossible not to suspect that his gas contained other adventitious oxygen, besides that derived from the dilute sulphuric acid which went into the gas-evolution bottle. Under these circumstances we thought we had better try and determine Dumas' adventitious oxygen as far as now possible ourselves, and we thus came to carry out the experiment referred to in the prefatory note as having given us the conviction that his correction was below the truth. This experiment (detailed hereafter as *Exp. I.*) was, of course, meant to be repeated, and it really was, but only after the intervention of a deal of other work. At the time it struck us that being, unlike Dumas, in possession of a method for the production of a current of absolutely oxygen-free hydrogen, the best thing we could do was to effect a series of quantitative syntheses of water by means of such gas, provided only we took care to maintain our critical attitude, and to do to our own work what we had originally proposed to do in reference to that of our great predecessor. As an important preliminary, we tried to ascertain whether it is possible to pass a current of hydrogen over a surface of red-hot glass without producing at least traces of water from it and the oxygen of the bases in the outer skin of the tube. For this purpose we carried out two series of experiments. In the first we passed the gas over relatively large surfaces of red-hot glass, and weighed the water produced from a known volume of hydrogen. In

the second experiment we aimed more directly at the probable magnitude of supposed error by carrying out a number of quantitative syntheses of water with, in general, large known volumes of hydrogen, but varying weights of oxide of copper. Supposing S to stand for the weight of oxygen used up in a given synthesis, W for the weight of water produced, and V for the number of litres of hydrogen which passed through the hot reduction-tube over and above the minimum which would have sufficed to convert those S grammes of oxygen into water, we have

$$W = kS + k'V,$$

and, theoretically, all that is required for the determination of the constants are two syntheses—one carried out with little, and the other with a large excess of hydrogen gas. But, in practice, it is better to determine the first constant k (which, obviously, if we weigh in air, is not the true value of $H_2O : O$) by means of a few experiments with relatively large quantities of oxide of copper and small excesses of hydrogen, and then to substitute this k in the calculation of syntheses made with small quantities of the oxide, to find k'. We, however, commenced with these latter determinations and, in their original interpretation, assumed that k was equal to 1·125 exactly, which, in a practical sense, was confirmed by the syntheses subsequently carried out with large weights of oxide of copper. These syntheses were quite completed and calculated when we made a very unwelcome discovery. To prepare for the (it does not matter now what) experiment, we had filled a Dumas system of hydrogen purifiers with the gas by means of a Kipp, closed the outlet, and left the apparatus in this condition over night. When we turned on the hydrogen on the following morning, the gas was found to smell so strongly of sulphurous acid that it would have been impossible not to notice it. We had made quite sure of the absence of this impurity from our vitrioled pumice; hence it was clear that it had been produced from the sulphuric acid by its prolonged contact with hydrogen. But Dumas' syntheses, as we are informed, always took some ten hours for their execution, hence his hydrogen must have been contaminated with sulphurous acid in all those cases at least in which he dehydrated it by means of oil of vitriol. We accordingly instituted a comparison of the results of these experiments with those which he arrived at when he dehydrated his gas with phosphoric anhydride, but we could not see any marked difference, and concluded that he used the more powerful dehydrator,

not by itself, but as an auxiliary to oil of vitriol. But, be this as it may, our own syntheses, although they were carried through in far less time, and although our vitrioled-pumice tubes were far smaller than Dumas', must in some measure have been infected by the same error. It was also clear that what had so far been put down by us as "adventitious oxygen O_x" had partly consisted of SO_2. We therefore made a series of critical experiments to determine the magnitude of the corresponding error, and, in our final series of syntheses, used fused caustic potash combined with phosphoric anhydride as sole dehydrators. After this digression we will now proceed to give a brief but sufficiently detailed account of all those of our experiments which bear more directly on the question under discussion.

I.—Determination of the Adventitious Oxygen in Dumas' Hydrogen.

Experiment I.—The apparatus used consisted of the following successive parts:—

(1) A Kipp, or a combination of two Kipps *(vide infra)*, charged with ordinary (we mean unboiled) dilute sulphuric acid.

(2) A close imitation of Dumas' set of purifying tubes for the hydrogen, namely:—

- (*a*) A tube charged with glass fragments and a strong solution of nitrate of lead.
- (*b*) A similar tube with solution of sulphate of silver. In these two tubes the volume of reagent was so adjusted that the gas had to bubble through a not inconsiderable column of liquid in the bent portion.
- (*c*) A third tube, of which the entrance limb was charged with fragments of pumice which had been boiled with solution of caustic potash, and the other with fragments of the fused reagent.
- (*d and e*) Two tubes charged with fragments of fused caustic potash.
- (*f and f′*) Two tubes charged with fragments of pumice soaked in oil of vitriol.*

* We will avail ourselves of this opportunity for stating how we prepared our vitrioled-pumice tubes. After having made sure of the absence of nitrogen-oxides and of sulphurous acid from our vitriol, a quantity of cut-up pumice was placed in a large platinum basin, covered with a large excess of the acid, and the acid then kept at a boiling heat until about one-half of it had volatilised. The whole was then allowed to cool under a bell-jar,

The only deviation from Dumas' design, which we allowed ourselves, consisted in this, that not having such very large U-tubes as he used at hand, we gave our tubes the form of rather flat Vs, as shown by Fig. 2 (p. 6).

Our tubes, however, had the same length as Dumas', so that the gas, as in his case, had to travel through about seven metres of purifying tubes on its journey.

(3) A tared "witness-tube" G, charged with vitrioled pumice.

(4) A combustion-tube, 370 mm. long and 15 mm. wide inside, charged with as thick a closely wound spiral of fine copper gauze as it would hold, and then drawn out at both ends to avoid the use of corks or india-rubber stoppers.

(5) and (6) Two U-tubes charged with vitrioled pumice; the first K was tared, the second L not, because it served only as a protection-tube against ingress of vapour of water from the atmosphere, or from No. 7, which was a large graduated "Pisani bottle," in which the outgoing gas was collected and measured.

The following shows the sequence of the apparatus :—

Kipp.	Dumas'. Purifiers.	U-tube, Vitriol.	Combustion Tube.	U-tube, Vitriol.	U-tube, Vitriol.	Pisani.
(1)	a, b, c, d, e, f, f'	G	Red-hot Copper.	K	L	(7)

In the actual experiment, the first step was to pass hydrogen through the Dumas' purifiers until the gas would have been deemed free of air for any ordinary purpose. The weighed witness-tube, G, and the copper-gauze tube were then attached, and after the copper had become red-hot, three further litres of hydrogen were sent through the apparatus to purge the copper from its water and oxygen. This being done, the weighed tube, K, and its protection-tube were attached, and the experiment continued until a sufficiency of hydrogen had accumulated in the Pisani. At the end, the tubes K and L were detached, the hydrogen in the former displaced by vitriol-dried air, and this tube weighed—the flames under the copper gauze tube being turned off immediately after the removal of K. As soon as the copper tube had cooled

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the surplus vitriol poured off, and the "vitrioled pumice" at once transferred to its U-tubes. When a tube had to be recharged, it was filled completely with fresh acid (previously boiled down in a platinum basin to expel more volatile impurities), allowed to stand for ten minutes, and the surplus vitriol then poured off. This washing process was repeated once, and the tube then deemed ready for use.

down sufficiently, the witness-tube G was detached and got ready for the balance. The volume of hydrogen operated upon amounted to twelve litres, which passed through in 3·5 hours. Tube G gained 0·2 mgm., tube K 3·45 mgm., or 0·288 mgm. per litre of hydrogen used.

Experiment II.—(Done long after I.) The apparatus used differed from that used in *I.* only in this, that in place of the copper-gauze tube we inserted the reduction-tube of our apparatus for the quantitative syntheses (L O in Fig. 3) full of reduced copper as it came from one of these. The volume of hydrogen operated upon was ten litres, and the weight of water produced from it = 7·65 mgm., but the witness-tube corresponding to G of *Exp. I.* gained 10·8 mgm.

Experiment III.—(Done immediately after II.) In this experiment we obtained 8·2 mgm. of water from 11 litres of hydrogen, or ·745 mgm. per litre. The witness-tube this time gained only 0·4 mgm.

Experiment IV. — (Done long after the preceding ones, and after we had come to observe the reducing action of hydrogen on oil of vitriol.) The apparatus consisted of the following successive parts:—

(1) A Kipp.

(2) The set of Dumas' purifiers up to the last caustic-potash tube, designated as "(2) *d*" under "*Experiment I.*" The two vitriol tubes, *f* and *f'*, were omitted.

(3) From *d* the gas passed on successively to a tared witness U-tube, K, charged with recently-fused caustic potash, and followed by a tared U-tube, P, containing pumice and phosphoric anhydride, respectively (*vide infra*, under "final syntheses"); the reduction-tube full of reduced metallic copper; a tared U-tube, K_1, and a witness U-tube, P_1, charged with fused caustic potash and phosphoric anhydride, respectively; a protection-tube containing fused caustic potash; and, lastly, the graduated Pisani bottle.

The exact *modus operandi* needs not be described; it suffices to state the results as follows:—The volume of hydrogen operated upon was 12 litres; the tubes K and P gained 1·5 and 0·2 mgm. respectively, the tubes K_1 and P_1 2·5 mgm. and *nil.* Hence water produced 2·5 mgm. or 0·208 per litre of hydrogen used.

Experiment V.—(Followed immediately after No. IV. and was done in exactly the same manner.) Increases of weight suffered by tubes K, P, K_1, P_1, = 0·7, 0·2, 2·3, 0·2 mgm., respectively; volume of hydrogen used = 12 litres; hence water produced per litre of hydrogen = 2·5 : 12 = 0·208.

Hence we had, for the potential water in Dumas' hydrogen, milligrammes per litre :—

Experiment	I.	II.	III.	IV.	V.
From O_2 and SO_2, ..	·288	·765	·745		
From O_2 alone, ...				·208	·208

In the case of Nos. II. and III. the gas probably was largely contaminated with sulphurous acid.

II.—ACTION OF HYDROGEN ON HOT GLASS.

A current of hydrogen, purified finally by being passed through a red-hot tube full of copper-wire gauze *(vide supra)*, was passed through a U-tube containing vitrioled pumice, thence through a combustion-tube 300 mm. long and 15 mm. wide inside, which was kept at a red heat by means of a combustion furnace, and, from this tube, into a graduated Pisani bottle; and this operation was continued until we felt sure from the volume of gas collected that the hydrogen, as it came out of the combustion-tube, must be as free as at all possible, of both water and oxygen (its liability to be contaminated with sulphurous acid was not yet known to us at the time). We then attached to the outlet of the combustion-tube two U-tubes charged with vitrioled pumice, of which the first was tared. From the second the gas passed into the Pisani bottle to be measured there. The experiment was continued until 12 litres of hydrogen had passed through the tared U-tube, which was then detached and weighed. In the first four experiments the combustion-tube was empty; in the fifth it was filled with fragments of combustion-tubing; in the sixth the combustion-tube was in the condition in which it had been left by the preceding experiment.

The results were as follows :—

Experiment	(1)	(2)	(3)	(4)	(5)	(6)
Time allowed for the 12 litres passing through,	2·5	1·5	3	3·25	3·50	3·5 hours.
Milligrammes of water obtained, ...	0·4	3·0	− 0·9	5·0	10·3	5·3 ,,

Experiments (1) and (3), as we see, yielded practically no water (in No. 3, indeed, the U-tube lost 0·9 mgm.); the 3 mgm.

in experiment (2) may be accounted for by the gas having passed through very rapidly, but the 5 mgm. produced in experiment (4) cannot thus be explained away. Suspecting that the water produced in the copper-gauze tube had not been completely retained by the U-tube provided for the purpose, we inserted a tared U-tube with vitrioled pumice after it, before proceeding to experiments (5) and (6); but this additional tube gained only 0·9 mgm. in (5) and 0·8 in (6). The obvious irregularities in the results are probably owing to a small quantity of sulphurous acid produced in the U-tube following the copper gauze; but, as this U-tube was only small, we have little doubt that the 10·3 mgm. of water obtained in (5) were at least partly produced by the action of the hydrogen on the hot glass. Admitting this, the fact that only 5·3 mgm. were obtained in experiment (6) is easily explained: the available stock of oxygen in the skins of the fragments had been largely exhausted by experiment (5). We will resume these experiments with perfectly oxygen-free hydrogen, such as we subsequently learned to prepare, as soon as we can find the time, because it is important to know whether or not red-hot glass is absolutely proof against hydrogen.

III.—Syntheses of Water effected with small weights of Oxide of Copper.

Referring to the section headed "*Our first series of Syntheses*" (and to Fig. 3) for a description of our apparatus and exact mode of operating, we at once pass to a statement of our results. A glance at the table shows that experiments (1) and (2) were absolute blanks, having been made with an empty reduction-tube. Under "Oxygen used" we give the loss of weight suffered by the oxide of copper, uncorrected for the displaced air. From it the "water due" is calculated by multiplication with 1·12537 (see under "First series of Syntheses"). "Surplus hydrogen" means the number of litres of hydrogen which were collected in the "Pisani" bottle while the gas streamed through the hot reduction-tube, uncorrected. "Water obtained" means the total weight of water obtained, uncorrected for the displaced air.

Experiment	(1)	(2)	(3)	(4)	(5)
Oxygen used, milligrammes,	0	0	291·95	324·52	415·8
Water due, ...	0	0	328·6	365·2	467·93
Surplus hydrogen used, litres,	5	3	10	10	9·5
Water obtained, milligrammes,	0·6	0·25	327·5	366·9	467·9
Surplus water, ...	0·6	0·25	− 1·1	1·7	− 0·03

Experiment	(6)	(7)	(8)	(9)	(10)
Oxygen used, ...	426·88	4230·25	2876·45	4805·25	621·2
Water due, ...	480·41	4760·6	3237·1	5407·6	699·08
Surplus hydrogen used, ...	9·5 (?)	3	3·5	3	5
Water obtained, ...	480·35	4761·1	3235·9	5406·6	700·3
Surplus water, ...	−0·06	0·5	−1·2	−1·0	1·22

SUPPLEMENTARY EXPERIMENTS.

(11) In this experiment the "Oxygen used" amounted to 411·9 mgm., and the reduction was effected with the least sufficient volume of hydrogen. The water obtained weighed 463·6 mgm., that is to say, 0·06 more than 411·9 × 1·12537. After the apparatus had been weighed, the parts were again put together, the copper was heated in a litre of hydrogen to make sure of its freedom from oxygen of any kind; only then the water-absorption tubes were appended, and the experiment continued until 10 litres of hydrogen had gone over the heated metal. Of the two vitrioled-pumice tubes which followed the reduction-tube, the first lost 4·45, the second gained 5·05 mgm., which anomaly we were not able to explain, and perhaps we had no right to look upon the net gain of 0·6 mgm., as representing the adventitious water produced from these 10 litres of hydrogen.

(12) Essentially a repetition of No. 11, except that only 3 litres of hydrogen were used in the second stage. "Oxygen used" = 474·35 mgm.; water due = 533·64; water got = 534·4; excess = 0·57 mgm. In the second stage the anomaly noticed in No. 11 again presented itself; the first of the vitrioled U-tubes lost 2·6 mgm., the second gained 3·3; net gain = 0·7 mgm.

(13) The copper resulting from (12) was left in the tube over night; it was then reheated in 3 litres of hydrogen, the water-absorption tubes attached, and the experiment continued until 11 litres of hydrogen had gone through the apparatus. The first U-tube lost 2·65 mgm., the second gained 2·55; net loss = 0·1 mgm.

(14) Oxygen used = 609·55 mgm., water due = 685·97; water obtained = 686·30; excess = 0·33 mgm. In the second stage 12 litres of hydrogen were used, the adventitious water amounted to − 1·55 + 2·8 = 1·25 mgm.

(15) and (16) In these experiments the oxide-of-copper tube was omitted and its place taken by a somewhat smaller tube drawn out at both ends, and charged with a scroll of platinum foil. The platinum was heated in the hydrogen for a whole hour before the

U-tubes for the absorption of the water were attached. The hydrogen from the copper-gauze tube passed through two successive U-tubes charged with vitrioled pumice, and the platinum-tube was followed by two similar tared U-tubes (I. and II.), and these again by a protection-tube to keep out the vapour of water from the Pisani bottle.

Experiment ...	(15)	(16)	(16) A
Hydrogen used, ...	10	11	12 litres.
Gain of U-tube I., ...	1·5	1·0	1·7 mgm.
Gain of U-tube II., ...	0·2	– 0·15	0·1 mgm.
Total water produced,	1·7	0·85	1·8 mgm.

(17) and (18) In these experiments an empty tube was substituted for the one containing the platinum foil; the procedure otherwise was the same as in the case of (15) and (16).

Experiment	(17)	(18)
Hydrogen used, ...	10	10 litres.
Gain of U-tube I., ...	1·1	1·3 mgm.
Gain of U-tube II., ...	–0·2	–0·5 mgm.
Total adventitious water, ...	0·9	0·8 mgm.

At the time when these nineteen experiments were made, we were still in ignorance of the fact that even in the cold hydrogen acts perceptibly on oil of vitriol with formation of sulphurous acid. We were, therefore, quite at a loss to explain the anomaly which presented itself in some of them, that certain U-tubes, which ought to have remained constant or gained weight, lost weight. On the whole, however, we took them as proving that in a synthesis of water carried out with, say 8 grammes or more, of oxygen, the adventitious water produced in the reduction-tube amounts to very little, provided the hydrogen which enters the reduction-tube is really free of oxygen; and ours, we thought, was because we had passed the gas through a tube full of red-hot copper gauze, followed by one or two U-tubes charged with vitrioled pumice.

Effect of Vitrioled Pumice on Hydrogen Gas.

To form an idea of the extent to which Dumas' syntheses may have been vitiated by the formation of sulphurous acid in the vitrioled-pumice tubes which he used for drying his hydrogen, we produced a continuous current of Dumas' hydrogen and determined the volume of a standard solution of permanganate which a given volume of the gas decolorised.

As a necessary preliminary, a current of hydrogen, which had been purified only by passing it through the acetate of lead, the nitrate of silver, and the caustic-potash tubes of the Dumas' set, was sent through a quantity of acidulated water which had been just barely reddened by addition of a drop of permanganate. After the gas had been going through for four hours, the reagent had turned brownish through partial reduction of the Mn_2O_7 to MnO_2; hence it was proved that pure hydrogen does not act sufficiently on permanganate to prohibit the projected method of analysis.

This point being settled, the two long vitriol tubes were appended to the Dumas' set, the hydrogen turned on, and kept going until all the air was sure to be expelled. The following set of successive apparatus (see Fig. 7) was then appended to the outlet of the last vitriol-tube:—A test-tube, B, containing some water and dilute sulphuric acid; a similar test-tube, C, charged with alkaline permanganate; two small successive U-tubes, D and E, charged, the first with fragments of caustic potash, the second with vitrioled pumice; and, lastly, a test-tube, F, similar to B and C. The test-tubes B and F both communicated with a burette containing standard permanganate. From the outlet of F the gas passed into a graduated Pisani bottle to be measured there. The apparatus being adjusted, hydrogen was passed through it, and the sulphurous acid contained in the gas titrated as it came by occasional addition of permanganate from out of the burette so as to maintain a distinct red coloration in the respective liquids. The maintenance of the end reaction, however, became more and more difficult as the experiment progressed, on account of the formation of precipitates of MnO_2 in the reagents.

In all, 12 litres of gas were passed through the apparatus, and, as far as we were able to determine the SO_2, it amounted to 4·8 mgm. in test-tube B, and to 0·48 mgm. in F. These numbers of course could be looked upon only as rough approximations. Hence,

In a *Second Experiment,* the sulphurous acid in the gas was determined more exactly by passing it through 8 c.c. and 4 c.c. of permanganate (1 c.c. = 5·572 mgm. of iron) contained in two successive Erlenmeyer flasks, B and C (substituted for the test-tubes previously used), besides some sulphuric acid, and determining the SO_2 absorbed at the end of the experiment by adding a known excessive weight of standardised ferrous sulphate and titrating

back with the permanganate. The tube F received 0·3 c.c. of permanganate, which were just decolorised by the 10 litres of gas which passed through the apparatus in 3 hours. The SO_2 absorbed in B and C amounted to 11·89 mgm., or to 1·19 mgm. per litre. Before this experiment was started the Dumas' set of purifying tubes, including the two terminal vitriol-tubes, had been standing filled with hydrogen for two days. Hence,

In a *Third Experiment*, those two tubes were emptied out, and the vitrioled-pumice fragments evaporated to dryness in a platinum basin; fresh vitriol was then poured on them, and the greater part of this again was chased away by evaporation. The pieces of pumice were then lifted out by means of a platinum forceps, replaced in their tubes, and the experiment then started without unnecessary delay. The residual vitriol in the basin was tested for sulphurous acid and found pure. In 3·5 hours 18 litres of hydrogen were passed through. The SO_2 formed amounted to 16·4 mgm., or to 0·91 mgm. per litre of gas.

Dumas' syntheses, as we are told, often took over twelve hours for their completion; hence, in all those cases in which vitriol was used as a dehydrating agent, the hydrogen must have been contaminated with sulphurous acid. For every two milligrammes of SO_2 which passed over Dumas' red-hot oxide of copper, the residual copper contained one milligramme of sulphur as Cu_2S, and the water collected included 1·125 mgm. of water produced from the oxygen of the sulphurous acid. Supposing the loss of weight suffered by the reduction-tube amounted to S, and the weight of SO_2 acting upon the oxide of copper was $= n$, the oxygen which produced the W parts of water obtained amounted to $S + \frac{n}{2} + \frac{n}{2} = S + n$ parts. Hence we have for the true value k_0 of the ratio $H_2O : O$,

$$k_0 = \frac{W}{S + n} = \left(\frac{W}{S} = k\right) \times \frac{1}{1 + n/S}$$

For a guess at the correcting factor, let us take $S = 8000$ mgm., and assume that the hydrogen used for the conversion of that oxygen into water amounted to 12 litres; whence $n = 12 \times 0{\cdot}91$ by Experiment III. Hence $1 : (1 + \frac{n}{S}) = 1 - {\cdot}001365$. Now, according to our calculation, Dumas' value for k, uncorrected for adventitious oxygen, was $=$ 1·12547; hence we have,

by the formula, $k_0 = 1 \cdot 12393$, and for $H_2 : O$ the number 0·12393.

Those of Dumas' syntheses in which phosphoric anhydride was used instead of oil of vitriol ought to be free of the sulphurous acid error, but we do not find any confirmation of this in his tabular statement of results; hence we presume that the phosphoric anhydride was used only as an auxiliary to oil of vitriol.

Some time after these experiments had been made, it struck us that we had better make sure of our theory of the effect of the sulphurous acid in the hydrogen on the results of a synthesis of water. After some pioneering work, the following method was adopted and carried out.

A combustion-tube, charged with pure reduced copper, was made to communicate, by its outlet end (indirectly *vide infra*), with a graduated Pisani bottle, and by its inlet end with a two-way glass stopcock communicating with a Kipp (charged with zinc and dilute sulphuric acid) in such a manner that we were able to let the hydrogen go by either of two prescribed ways (see Fig. 10). The hydrogen was purified by passing it, first through a large tower filled with pieces of caustic soda, and then through a tube full of red-hot copper gauze. If the gas went one way, it entered the combustion-tube containing the reduced copper as it was; if it went the other way, it had to pass through a two-litre bottle B, which was almost completely filled with a solution of sulphurous acid. A special experiment showed that the gas, as it came out of the sulphurous acid bottle, contained 3·16 mgm. of SO_2 per litre.

In the experiment, the first step was to pass pure hydrogen over the red-hot reduced copper. A test-tube, containing a quantity of acidulated water coloured just perceptibly with permanganate, was then inserted between the outlet end of the reduced-copper tube and the Pisani bottle, and seven litres of hydrogen, contaminated with SO_2 were passed through. The permanganate retained its colour, showing that no SO_2 got past the red-hot copper. In a subsequent experiment, the combustion-tube was filled with oxide of copper, and an apparatus for the collection of the water formed inserted between the oxide-of-copper tube and the test-tube. As in the former case the reduction was started with pure hydrogen, impure hydrogen was then made to go through, and pure hydrogen again substituted for it at the end. About 50 grammes of oxide of copper were used, corresponding to 10 litres of hydrogen. The permanganate in the test-tube retained its

colour, and the water collected as such was found free of sulphurous acid. This shows that neither copper nor oxide of copper allows any sulphurous acid to pass, or, in other words, that the sulphurous acid is decomposed completely with formation of sulphide of copper.

In our calculation of the presumable effect of the sulphurous acid on the result of Dumas' syntheses, we neglected the error caused by the sulphurous acid contained in the surplus hydrogen which goes out of the water-receptacle through the action of the vitriol contained in the drying tubes included in the latter. As the quantitative effect of *this* sulphurous acid is difficult to predict, we tried to determine it by direct experiments. But, in doing so, we had our eyes more on our own "First series of Syntheses" (*vide infra*) than on Dumas'; we therefore used small U-tubes charged with vitrioled pumice, such as we had used in our own syntheses. A current of hydrogen, purified by means of a tower of caustic soda and red-hot copper gauze, was passed through (1) a tube containing phosphoric anhydride; (2) a similar tube charged with the same reagent; (3, 4, and 5) three successive U-tubes charged with vitrioled pumice; and from the last vitriol-tube the gas passed into the graduated Pisani.

Experiment I.—Volume of hydrogen used = 3·5 litres. Temperature = 17·5 – 17·8°C. The second P_2O_5-tube gained 0·1 mgm., the three vitriol-tubes

the three vitriol-tubes	(3)	(4)	(5)
lost	0·3	1·6	4·8 mgm.

Experiment II.—Volume of hydrogen used = 6 litres. Temperature = 16·5 – 17°C. The second P_2O_5-tube gained 2·3 mgm., the three vitriol-tubes

three vitriol-tubes	(3)	(4)	(5)
lost	0·3	1·0	0·0 mgm.

Experiment III.—In this experiment a tube charged with recently-fused caustic potash was substituted for the first phosphoric acid tube. Volume of hydrogen used = 9·5 litres. Temperature not observed. The P_2O_5-tube gained 0·3 mgm., the vitriol-tubes

	(3)	(4)	(5)
lost	1·2	1·9	– 0·1 mgm.

The tubes (3) and (4) correspond to the two vitriol-tubes which were attached to our water-receptacle, and weighed before and after the synthesis. The conjoint loss of these two tubes in

Experiments ...	I.	II.	III.
was	1·9	1·3	3·1 mgm.

The error, as we see, is not very considerable, but it is too great to be neglected, and with Dumas' very large U-tubes, it must have been greater than in our case. That the witness-tube in Experiment I. lost almost 5 mgm. is an anomaly which we cannot explain.

Of the several sources of error in Dumas' experiments, which we are able to see, only one remains to be considered. We allude to the unavoidable presence of *occluded hydrogen* in the metallic copper produced. Dumas was quite alive to this source of error, and, indeed, caused Melsens to determine its magnitude. In one of Melsens' experiments the metallic copper produced from 300 grm. of oxide by reduction in hydrogen, when re-burned in oxygen, gave 65 mgm. of water = 7·2 mgm. of hydrogen or 0·000119 grm. per gramme of oxygen in the oxide of copper started with. Dumas states that once, when the reduction of the oxide was effected at an exceptionally low temperature, the copper contained 0·0002 of its weight of hydrogen. In this case the weight of hydrogen per gramme of oxygen in the original oxide was 0·0008 grm.

In the course of our first series of syntheses we sometimes determined the occluded hydrogen in the metal produced, and we may as well communicate the results now. After the completion of a synthesis of an evening, the reduction-tube was stopped up and kept over night. On the following morning the tube was reweighed, to make sure that it had not changed weight, or to determine any change that might have taken place. It was then heated in a current of air, dried by means of vitrioled pumice, until it had suffered a very far-going oxidation, the water being collected in a tared U-tube charged with vitrioled pumice and weighed. In the following table the headings refer to the pages of our Journal.

Page, ...	44	45	67	71
Oxygen used as oxide of copper, grammes, ...	2·876	4·805	4·262	5·539
Water formed in milligrammes, ...	0·3	0·1	2·4	2·9
Occluded hydrogen,	·03	·01	·27	·32
Hydrogen, per gramme of oxygen, milligrammes,	·01	·002	·0626	·0582

Page, ...	75	77	79	89
Oxygen used, ...	10·372	10·526	10·424	15·460
Water produced, ...	3·7	2·7	2·1	7·2
Occluded hydrogen, ...	·41	·3	·23	·8
Ditto, per gramme of oxygen, milligrammes,	·0396	·0284	·0224	·0518

Taking δ as a symbol for the occluded hydrogen per gramme of oxygen used apparently as oxide of copper, if the water obtained in the synthesis per 1 grm. of loss of weight suffered by the oxide of copper was $= k$, the true value k_0 of the ratio $H_2O : O$ is $k\ (1-\delta)$. Now, our highest value for $k\ \delta$ was ·000071 grm., hence $k - k_0 = \cdot000071$. Supposing, for instance, the true k_0 were equal to 1·125 then $k = 1\cdot125071$, corresponding for $O = 16$, to $H = 1\cdot000568$.

But our average value for δ is only ·0000344, hence $k - k_0 = \cdot000039$, corresponding to $H = 1\cdot000312$. From Melsens' experiment with the metal from 300 grm. of oxide we should have $H = 1\cdot00100$. Our conclusion at the time was that the influence of the occluded hydrogen may safely be neglected.

To sum up: Assuming Dumas' hydrogen contained the proportion of atmospheric oxygen corresponding to our determination V, page 15, and 0·91 mgm. of SO_2 per litre (see page 20), then his (uncorrected) value $k = 1\cdot12547$ for $H_2O : O$ is liable to the following corrections:—

(1) On account of the oxygen, ...	− ·000287
(2) On account of the sulphurous acid, ...	− ·00154
Total, ...	− ·001827

Hence, corrected value $k = 1\cdot12364$. All this on the assumption that the water-weights of Dumas are the *true weights*. If he forgot to reduce these to the vacuum, the corrected k must be corrected up by $0\cdot001227 \times 1\cdot12547 = 0\cdot00138$, so that we have, for the fully corrected k' the number 1·12502; whence, for $O = 16$, $H = 1\cdot00016$!

Let us now pass to a consideration of Erdmann and Marchand's work.

Erdmann and Marchand's Work.

There is no need of our here repeating what was said in the prefatory note, page 35. We therefore pass at once to an account of our experiments on the dehydrating powers of chloride of calcium and caustic potash. Our apparatus consisted of the following successive communicating parts:—

I.—A Pisani gasholder, containing a little over 12 litres of air, shut up over dilute solution of caustic potash or soda, followed by—

A. A large tower charged with fragments of recently-fused caustic potash.

B. A tared U-tube, charged with the same reagent.

C and *D*. Two U-tubes, charged with vitrioled pumice; both tared.
E. A wash-bottle, containing water.
F and *G*. Two U-tubes charged with fragments of recently-fused chloride of calcium; both tared.
H. A U-tube, charged with vitrioled pumice; tared.
K. A similar tube, untared.

The U-tubes intended for the determination of the weights of absorbed matter, were all tared, not with weights, but with similar tubes of as nearly as possible the same displacement. Each tare-tube contained a quantity of selenite adjusted so that it displaced very nearly the same volume of air as the chloride of calcium or caustic potash in the working tube. Our U-tubes were all of that now popular kind in which the gas enters and goes out through laterally-soldered-in short tubes, and the orifices are provided with perforated ground-in hollow glass stoppers, so that one can close either or both sides by a turn of the stopper, or open them to admit the respective current of gas.*

The necessary tarings having been effected, and the apparatus put together, the air from the Pisani was turned on and made to pass through the apparatus at a suitable rate, until a sufficient volume had accumulated in the graduated Pisani bottle. The following diagram will facilitate the reading of the tables of results:—

Pisani	KHO	KHO	H_2SO_4	H_2SO_4	Water	$CaCl_2$	$CaCl_2$	H_2SO_4	H_2SO_4
I.	A	B	C	D	E	F	G	H	K

FIRST SET.

Experiment	(1)	(2)	(3)	
Temperature,	12° - 14°	12·7° - 14°	14° - 16°	
Volume of air,	12	2·5	11	litres.
which passed in	4	1·25	2	hours.
KHO, B, gained	0·6	0·15	- 0·2	milligrammes.
Vitriol, C, gained	1·35	0·15	0·1	,,
		After Water.		
$CaCl_2$, F, gained	—	—	70·4	,,
$CaCl_2$, G, gained	0·2	Nil	12·8	,,
Vitriol, H, gained	2·2	0·45	2·5	,,

From these numbers we see that the caustic potash tower dehydrated the air so completely that the potash U-tube following it had little or nothing to do; and, assuming oil of vitriol to be a

* Both remarks apply to the vast majority of the respective experiments reported on in the preceding section.

perfect dehydrator for gases, that in experiments (2) and (3) at least, the caustic potash dehydrated the air as good as completely. The fused chloride of calcium dehydrated the moist air coming from E so completely, that only 0·183, 0·180, 0·227 mgm. of water were left in every litre of air. In a corresponding experiment by Fresenius the residuum of water amounted to 0·97 mgm. per litre. (See page 179 of his memoir.) Fresenius says that his chloride of calcium was fused and put into the tubes while still hot. This is exactly what we did, except that we allowed our preparation to cool before filling the tubes with it; hence we are at a loss to explain why our tubes worked so much better than his. But we had no doubt in our mind that Fresenius' chloride of calcium was a fair preparation and took it to be a fair presumption that Erdmann and Marchand's preparation was no better than that of Fresenius. We therefore spoiled our chloride of calcium expressly by passing moist air through it until it had gained 130 mgm. in weight, and then resumed our experiments. In experiments (4), (5), (6) the wash-bottle E and the two chloride-of-calcium tubes were immersed in a water-bath kept at 25°. In experiments (7) and (8) only the wash-bottle and the first chloride-of-calcium tube were kept at 25°, the second $CaCl_2$ tube was not artificially heated or cooled.

SECOND SET.

Experiment	(4)	(5)	(6)	(7)	(8)	
Temp. of Air,	13·5–16·5	13–16	11·5–15°	11–15°	—	
Temp. of water kept at 25° throughout the series.						
Vol. of Air,	10	10	10	10	10	litres.
Time,	2	2·25	2·25	2·25	2	hours.
Gain of B,	0·6	0·5	0·3	0·5	*	mgm.
Gain of C,	0·5	–0·1	0·3	0·3	*	mgm.
After Water.						
Gain of F,	122·8	129·6	114·8	125·0	*	mgm.
Gain of G,	14·0	9·3	2·9	6·6	2·0	mgm.
Gain of H,	6·3	5·0	7·8	6·0	5·0	mgm.
Hence, 1 litre of chloride of calcium dry air contains—						
Of water,	0·63	0·5	0·78	0·60	0·5	mgm.
While 1 litre of KHO-dry air contains—						
Of water,	0·05	0·0	0·03	0·03	*	mgm.

* Not determined on account of want of time.

We deem it worth while to state that, even after the eighth experiment, the chloride of calcium in the outlet limb of tube G was still glassy in appearance, and that even that in the inlet end was not visibly spoiled. What we mean to say is that it would have passed in any laboratory for fair enough chloride of calcium for ordinary work. From our experiments and Fresenius' conjointly, we derive the conviction that a gas which has been dried by means of apparently well-conditioned chloride of calcium may contain as much as 1 mgm. of water per litre. Fused caustic potash, on the other hand, dries a gas quite completely. We assert this the more confidently, as our second series of syntheses (which came long after the experiments now under discussion) proved to us again and again, that a gas, which has passed over a long enough column of recently fused caustic potash, *gives up no water even to phosphoric anhydride.*

On these facts we base the following hypothesis concerning Erdmann and Marchand's work :—In their last four experiments (and only these need be taken into account), the hydrogen which entered their reduction-tube was free of any kind of oxygen, because they avoided the use of oil of vitriol as a dehydrating agent, and passed their gas over red-hot metallic copper before it reached the final dehydrator. But the surplus hydrogen which passed through the reduction-tube at the end of the experiment, and the air which followed it, carried away with them an appreciable weight of vapour of water, because there was only a chloride-of-calcium tube at the outlet of the water-receptacle to catch it. Assuming now, that they used (let us say) 3 litres of surplus hydrogen and 3 litres of air, for every 8 grammes of oxygen used as oxide of copper, and that their chloride of calcium was at a par with Fresenius' as a dehydrator, about 6 mgm. of the water which they produced failed to find its way to the balance. Now, their reported water-weight was almost exactly = 9·000 grm. per 8 grammes of oxygen; hence their total water actually produced was equal to 9·006 grm.; and hence their real value for H (if O = 16), is 1·006, and not 1·000.

Dumas produced too much water, but may have under-determined its weight by forgetting the vacuum-reduction. Erdmann and Marchand produced the correct amount of water and weighed what they had on the balance correctly, but they lost some of their water before it came to the balance.

We will now pass to our own first series of syntheses which, as

may be remembered, was made before we had discovered the reducing action of vitrioled pumice on hydrogen gas.

Our First Series of Syntheses.

In it our original intention was to set up as close an imitation of Dumas' apparatus as we might be able to construct, and, by means of it, to carry out a few syntheses exactly in his style, in order to fortify our position as critics of his work, and, in the most direct manner possible, to decide the important question, whether he really had, as we suspected, forgotten to reduce his water-weights to the vacuum. But we soon came to give up this idea, for this reason amongst others, that we had not a high-class air-pump at our disposal, and *without such an instrument, we thought at the time,* we should not be able to weigh our oxide of copper and metallic copper *in vacuo,* as he did. We had to modify Dumas' method so as to bring it within the range of our resources, and this being so, we thought we might as well try and improve upon it, by utilising our method for the production of absolutely oxygen- (*i.e.,* O_2-) free hydrogen, and by working on a smaller scale. We were, and to this day are, convinced that Dumas positively lost precision by working with such extraordinarily large quantities of oxide of copper, that he had to execute his final weighings after having become exhausted by "from 15 to 20 hours" of continuous work. We have no doubt, in our own minds, that our results are at least as close approximations to the truth as his, although we worked on such a small scale that we were able to carry out all our weighings with a balance of only "100 grammes carrying power,"* and were able to complete a synthesis without working longer than from about nine in the morning to 5 – 6 in the evening. Dumas tells us that his nineteen published experiments correspond to no less than 40 to 50 such experiments actually made, which means about one failure for every two attempts. Of the 13 actual syntheses which we made, only *one* was a breakdown, and *one* other had to be rejected on account of unobserved blunders.

For the production of the necessary continuous supplies of hydrogen we used two "Kipps," which were coupled together by means of one of those admirable two-way cock arrangements of Messrs. Greiner and Friedrichs, so that we were able to use either one or the other, and, supposing one to be exhausted, to re-charge it, while the other was working. Let us add at once that we never

* The actual charge, it is true, sometimes rose to 120-130 grammes.

had occasion to do this. In a few of the first trials (with small quantities of oxide of copper), we used 10 per cent. hydrochloric acid for liberating the gas, but all the later experiments were made with 20 per cent. sulphuric acid. For the purification of the the gas, we used, at first, to pass it successfully through (1) cotton wool; (2) a U-tube charged with vitrioled pumice; (3) a combustion-tube 370 mm. long and 15 mm. wide inside, containing as thick a closely-wound spiral of fine copper gauze as it would hold, and drawn out at each end to avoid the use of corks. This tube was kept at a red heat during the progress of an experiment, and served to eliminate the antimony, arsenic, and sulphur, which the gas contained, besides converting the oxygen of the unavoidable trace of air into water, which latter was caught in a U-tube charged with vitrioled pumice, attached to the outlet. The principal impurity in our zinc was antimony; we therefore, in our later experiments, purified our gas by sending it from the "Kipp" direct into a large tower (210 mm. high and 35 mm. wide), filled with fragments of caustic soda, which converted almost the whole of the antimoniuretted hydrogen into a black deposit of SbOH. From the soda-tower the gas passed into the tube containing red-hot copper.

The Oxide of Copper was prepared from chemically pure copper foil by cutting it up, placing it in a Berlin basin, and, in it, heating it in a muffle for a day or two. The muffle was new, and, during the progress of the research was never used for any other work. The only impurity which the oxide could have contained is sulphur (from the gas-flames); we therefore tested about 10 grammes of it by treating it with pure fuming nitric acid, &c., and searching for sulphuric acid by means of chloride of barium. From the whole of the 10 grm. of oxide used, only a barely visible trace of a precipitate was obtained, which amounted to no more than the one seen in the "blank" which accompanied the experiment proper. Oxide of copper produced in the way described never contains the full proportion of oxygen demanded by the formula CuO, but this does not matter; over the oxide produced by heating the nitrate it offers the inestimable advantage of not being hygroscopic. When we had used up the first supply of oxide, the metal obtained from it was re-oxidised in the muffle, which took far less time than the oxidation of the original compact metal.

Our apparatus is represented in the Figs. 3.1, 3.2, 4, and 5.

The reduction-tube, L, was made out of a piece of wide combustion-tubing. When used, it was heated in a magnesia-bath by means of a set of Bunsens, a roof-shaped chimney made of asbestos pasteboard serving to reflect down the heat on the top part of the tube. In the original apparatus this tube was connected with the outlet of the preceding vitriol-tube by means of an india-rubber stopper, which, as we may state, was taken out before the apparatus went to the balance. But we subsequently discarded the stopper and substituted for it the arrangement shown more clearly in Fig. 5, for the explanation of which it suffices to state that the joints at K and at k′ were made tight by means of strips of warmed non-vulcanised sheet india-rubber wound round and secured by wire ligatures on both sides.

The water-receptacle, P, was made out of a fractionating flask. Fig. 3.2 shows its construction and mode of connection with the reduction-tube. The tube, d, is fixed in its place by means of a cork, g, which terminates a little below the edge of the neck of the flask so as to form a cup, which is filled completely with sealing-wax, so as to avoid contact between air and cork. The outlet of the reduction-tube O in Fig. 3.1, and a in Fig. 3.2, projects into the tube d, the joint at b being made tight by means of a wired-on band of black sheet india-rubber, as in the case of the improved form of the reduction-tube entrance. After a few experiments had been made with the apparatus as described, we found that, sometimes, a sublimate of water settled down in the annular space between d and a. To prevent this inconvenience, we provided the arrangement represented in Fig. 6. H is the last drying-tube preceding the reduction-tube. The original intention was to close the stopcock, b, whenever a sublimate of water shows itself between a and d, and let the gas go through the side-tube, c d e f. But we subsequently found it easy to adjust the cocks b and d, so that a small portion of the hydrogen went constantly through the side-tube and swept down any steam present within that annular space that might otherwise have condensed into a sublimate of water.

The weighings were effected by means of that very excellent Oertling's "14-inch," which one of us described in the *Zeitschrift für Instrumentenkunde* some years ago, and a set of iridio-platinum weights from Johnson, Matthey, & Co., which were adjusted by Oertling and subsequently readjusted by one of us (W.D.), with the co-operation of his then assistant, Mr. Barbour. To reduce the

uncertainties of the weighings to a minimum, the several portions of the apparatus were tared with somewhat lighter similar apparatus of as nearly as possible the same displacement, so that the weight-standards lying on the pan represented little more than the net weight to be determined.

In the execution of the syntheses the exact *modus operandi* was not always exactly the same. Little improvements were introduced as our experience expanded, but there would be no use in here giving the history of our apprenticeship; it suffices to describe the *modus operandi* in its latest form, which, indeed, was employed in all the actual syntheses, intended for the calculation of the value $H_2 : O$.

The first step in each case was to start the hydrogen, and let it go through the purifiers, the copper-tube being meanwhile kept at the ordinary temperature. The reduction-tube was then charged with a suitable quantity of oxide of copper, and prepared for the balance by heating the oxide in a current of vitriol-dried air, until every trace of moisture was sure to be expelled. The tube was then allowed to cool in dry air, closed at both ends, and next kept immersed in a water-bath of the temperature of the balance room, beside the tare-tube, until both could be assumed to have acquired that temperature. The two portions of the apparatus were then taken out of the bath, wiped dry with a towel, and suspended at the balance, the working-tube at the left end and the tare-tube at the right, a special tare equal in weight to the stopper and cap on the working-tube being placed in the right pan. After some time the working-tube was opened for a second, to bring its atmosphere to the pressure of the air outside, and equilibrium established by means of weights. Some ten minutes later, the weights were re-adjusted, and this operation was repeated until the state of equilibrium had become permanent, which always was the case after a short time—thanks chiefly to the water-bath which had established equilibrium of temperature. At the end, the cap and stopper were removed from the working-tube, and the corresponding tare from the right pan, and the balance made to vibrate two or three times to obtain the exact value of the weight to be determined. A similar method was used for the water-receptacle and the U-tubes, which were tared next. During one of the periods of rest involved in the tarings, the gas lamp under the copper wire-gauze tube was lighted, and the hydrogen allowed to stream through the hot tube till all the oxygen and moisture of the

gauze could be assumed to be removed. Only then the vitriol tube H was attached to the end of the copper-gauze tube, and the apparatus thus made ready for the joining on of the reduction-tube and the water-receptacle. The outlet of the last U-tube S had a long india-rubber tube attached to it, so that any gas that streamed out there could be collected over water in a graduated Pisani bottle (Fig. 1) provided for the purpose. The joints between P and R, and between R and S, were made with best india-rubber tubing in such a way that the two glass tube ends within the india-rubber almost touched each other, and the joint secured on both sides by ligatures of copper wire.

After attaching the oxide of copper and the water-receptacles, several litres of hydrogen were allowed to pass through the cold apparatus to make sure of every trace of enclosed air being eliminated, and only then the gas under the reduction-tube was lighted. As soon as the oxide of copper comes up to a certain temperature, which lies below redness, the hydrogen current collapses—the gas being converted completely into water by the first short layer of oxide which it strikes against. While the formation of liquid water progresses, the water-receptacle lies in an ice-bath to minimise the weight of water which passes into the U-tube; but as soon as the oxide appears to be completely reduced, a water-bath of the temperature of the laboratory is substituted, so that as little as possible of the vapour of water in the apparatus is driven out into the air, while the water expands in rising up to the temperature of the balance.

The experiment requires constant attention, but could not be said to be difficult of execution. When all the oxide of copper has apparently suffered reduction to metal, the process is continued for a while to make sure that the reduction really is complete; the lamps under the magnesia-bath are then turned off, and hydrogen is allowed to pass through the apparatus until the copper is quite cold. The reduction-tube and the water-apparatus are then detached from the rest of the apparatus, and prepared for the balance by passing a current of vitriol-dried air through them to expel the hydrogen. The water-receptacle is then closed by an india-rubber cap at the outlet end, and by means of a closely fitting though not ground-in stopper, as shown by Fig. 4, the reduction-tube by its stopper and cap as explained before, the U-tubes by turning their stoppers. The weighings are effected in the way already explained.

Before attempting a real synthesis we carried out those "blanks" and *quasi*-"blanks" which were detailed in the section on Dumas' work, and, to us, served as useful rehearsals.

The results of our syntheses proper are given in the following table, which includes all the experiments which we made, except one or two which had to be stopped in their middle on account of breakdowns; the table, in other words, includes all those of our experiments which were really completed. In a publication like the present, it is not right to exclude any experiment or experiments, because the result is not to the taste of the experimenter; but, on the other hand, *he* is the only person competent to say which of the results should be "blackened."

As our table gives only the net weights, we will give all the details of one of the experiments:—

Experiment VIII.

Weight of reduction-tube + oxide of copper, ...	tare + 36·3740 grm.
,, + metallic copper, ...	tare + 19·2814
Hence, oxygen S, uncorrected, ...	17·0926
Water-flask after the experiment, ...	tare + 25·3239
,, before ,, ...	tare + 6·1363
Hence, liquid water, ...	$19·1876 = w_0$
First U-tube following the water-flask, after the experiment, ...	tare + 9·1488
The same before the experiment, ...	tare + 9·0970
Hence, condensed steam, ...	$0·0518 = w'$
Second U-tube (H_2SO_4), after the experiment, ...	tare + 2·6812
The same, before, ...	tare + 2·6805
Hence, condensed steam, ...	$0·0007 = w'$
Third U-tube (P_2O_5), after, ...	tare + 1·0499
The same, before, ...	tare + 1-0496
Hence, condensed steam, ...	$0·0003 = w'''$

Hence, water-weight uncorrected $= w_0 + w' + w'' + w''' = 19·2404 = W$

Oxygen-weight uncorrected $= S =$ ... ... 17·0926

Hence $W - S$ (Hydrogen) $=$ ... ... $2·1478 = h$

Hence $h : S = 0·12566$, whence (for $O = 16$) H 1·00526

In this experiment, for the first time, phosphoric-anhydride tubes were used in conjunction with vitriol-tubes for drying the gas after leaving the copper-gauze tube and water-receptacle, respectively. In all the preceding experiments, only two successive U-tubes with vitriol-ed pumice were used at either place.

The above number (w‴) shows that, in Exp. 8 at least, the P_2O_5-tube might have been dispensed with.

In the following table the headings of the columns refer to our Journal; S stands for loss of weight suffered by the oxide of copper through its reduction to metal; w_0 for the weight of the liquid part of the water; w for the water collected in the U-tubes attached to the water-receptacle; W for $w_0 + w$; H for the uncorrected atomic weight of hydrogen referred to $O = 16$.

Table of Results.

Page	68	70	72	74	76	78
No.	(1)	(2)	(3)	(4)	(5)	(6)
S	4·26195	6·71315	5·53935	10·03585	10·3715	10·5256
w_0	4·7604	7·5038	6·20145	11·20945	11·6055	11·7933
w	·0376	·0465	·03575	·0838	·0673	·0500
W	4·7980	7·55025	6·2372	11·29325	11·6728	11·8433
H	1·0061	0·9977	1·0080	1·0024	1·0037	1·0015

Page	80	82*	84	86	88	90	92
No.	(7)	7.a	(8)	(9)	(10)	(11)	(12)
S	10·4243		17·0926	18·5234	16·2367	15·4598	17·11485
w_0	11·6902		19·1876	20·78495	19·09975	17·3691	19·2266
w	·0415		·0528	·0494	·04065	·03325	·0365
W	11·7317		19·2404	20·83435	19·1404	17·40235	19·2631
H	1·0033		1·00526	0·9981	1·4307	1·00527	1·0041

* A break-down.

Summary.

Uncorrected Values for H found.

(No. 10 excluded).

No.		H.		No.		H.
2	...	0·9977	...	12	...	1·0041
9	...	0·9981	...	11	...	1·0053
6	...	1·0015	...	8	...	1·0053
4	...	1·0024	...	1	...	1·0061
7	...	1·0033	...	3	...	1·0080
5	...	1·0037				

Mean of the 11 values = 1·0032.

Probable error of a single determination = $\pm$ ·0021.

Probable error of the mean = $\pm$ ·00064.

The value W:S, even if taken in its empirical sense, is infected with an error which we had no idea of when the experiments were

made: we refer to the presence of sulphurous acid in the hydrogen used. The quantity of this impurity per litre of gas used could not have been as great as in Dumas' case, because the two vitriol-tubes which followed our copper-gauze tube were very small compared with Dumas'. To form an idea of the probable magnitude of our error, we calculated the experiments tabulated under heading III. on pp. 16—18 as so many determinations of the sulphurous acid per litre of total hydrogen used, taking every gramme of oxygen used in a synthesis, as corresponding to 1·5 litres of hydrogen, measured moist over water at (we said) 15° and 748 mm. dry-gas pressure. The results varied from a very small negative quantity to 0·217 mgm. as a maximum, the mean was 0·082 mgm.* Assuming that the SO_2 in all the hydrogen used in the syntheses just tabulated, amounted to 0·2 mgm. per litre we arrive at $H_2O : O = 1{\cdot}12506$, or $H = 1{\cdot}0005$ as corrected numbers. But 0·2 mgm. per litre is more than the hydrogen can be assumed to have actually contained; hence the proper mode of interpreting the result, is to say that the true value for W : S lies somewhere between 1·12506 and 1·1254, or that of H between 1·0005 and 1·0032. But, whichever value we choose, we must correct it for the air displaced by the copper and copper oxide, and for the air displaced by the water. Strictly speaking, each of our experiments should be corrected by itself, but, considering that our values for H oscillate between 0·9977 and 1·0080, it suffices to correct their mean, or to view the eleven experiments as, so to say, *one* experiment and correct its result.

In the eleven experiments which we allowed to vote, the total quantities of oxygen, water, and, by difference, hydrogen found, were as follow :—

OXYGEN.		WATER.		HYDROGEN.
126·0624	...	141·8667	...	15·80435

Or, reducing to 1 grm. or 8 grm. of oxygen, respectively—

1	...	1·125369	...	0·125369
8	...	...	...	1·00295

Leaving the sulphurous acid on one side for a moment, we must now reduce both the oxygen and the water to the vacuum.

* The determinations here referred to have since been re-calculated, and suffered considerable corrections, yet we retain the result, feeling sure that 0·2 mgm. of SO_2 per litre is about as good a guess as it is possible to make.

The Oxygen.—Duplicate determinations of the specific gravities of the oxide of copper used, and of a specimen of the reduced copper obtained in an experiment gave the following results:—For the copper, 8·6959 and 8·7074—mean 8·7016; for the oxide, 6·1417 and 6·1420—mean 6·1418. In all the four determinations the temperature was 15°; yet, we may, without committing a serious error, read the specific gravities as giving the weights of 1 c.c., and take the volume of 1 grm. of oxide of copper as = ·16282 c.c. and the volume of 1 grm. of metallic copper as = ·11492 c.c. The oxide of copper, however, was not pure CuO, but something between it and Cu_2O. The composition of the oxide, passing from experiment to experiment, was not by any means constant, but in one it was ascertained to correspond to the formula O + 1·0985 Cu. Assuming this formula and the above specific volumes to hold all round, we have, per 16 grm. of oxygen, for the volume of the oxide of copper, 14·850 c.c., for that of the metal 8·912 c.c., hence for the volume of the oxygen 5·938 c.c.; but this volume of air of 15° and 760 mm. pressure weighs 5·938 × 1·2267 mgm. Hence, for 1 grm. of oxygen, the correction is = + 0·4553 mgm., and this added to the above 1 grm. gives 1·000455 grm. as the true weight of the oxygen. The 1·125 grm. of water displace 1·38 mgm. of air, hence the true weight of the water = 1·12675 grm. Hence we have, for 1 grm. of oxygen, 0·12624 grm. of hydrogen, and for 8 grm. of oxygen, 1·0099 = H grm. Allowing ·0014 for the sulphurous acid, we have H = 1·0085 ± ·0014 on account of the *uncertainty* in this correction. But, unfortunately, this is not the whole of the uncertainty, for this reason, in the first instance, that the oxide of copper and the metallic copper must both be presumed to have contained absorbed gases which were weighed as so much oxide and metal, respectively. Hence, when we came to carry out our second series of syntheses (which we projected as soon as we had discovered the reducing action of hydrogen on vitriol), we decided upon weighing our oxygen in the Dumas fashion, and did so (*vide infra*); and, after the completion of that second series, it struck us that we might utilise the reduction-tube used in it for a summary determination of the *full* correction which the oxygen-weights of the first series are liable to. For this purpose 115 grammes of the kind of oxide of copper which had been used in the second series were placed in the reduction-tube and subjected to exactly the same sequence of operations as would have been

involved in a synthesis of water, with this difference, only, that the oxide as well as the metal was weighed twice, namely, once in air against an open tare-tube, and once *in vacuo* against another, close, tare-tube. In the case of the metal the weighing in air came last, and it must be stated that the hydrogen-vacuum was *just undone and no more* by letting in dry air before the tube went to the balance. Two experiments were made in this manner, each with very nearly, but not exactly, 115 grm. of oxide. The results were as follow :—

Weight of Oxygen found.

Experiment	I.	II.
S, from weighings in air, uncorrected,	21·5128	20·6520 grm.
S_0, by weighings *in vacuo*, ...	21·5322	20·6727 grm.
Hence, $S_0 - S =$	·0194	·0207 grm.

—or, taking for each of the two quantities, the mean of the two experimental numbers, $S = 21{\cdot}0824, S_0 = 21{\cdot}10245, S_0 - S = {\cdot}02005$, whence $\frac{S_0 - S}{S} = {\cdot}000950$, or ·950 milligramme, for the weight of air displaced by 1 gramme of oxygen; and it is perhaps as well to note that the result would have been the same, practically, if the metal, previous to its first weighing, had been allowed to take up a few milligrammes of atmospheric oxygen. The above calculation, based on our determinations of the specific gravities of the oxide and metal, gave only ·4553 mgm.; a very considerable difference, which cannot be explained by observational errors, nor by the admitted fact that the oxide used throughout the syntheses was not constant in its composition, and even that used for the specific gravity determination was not proved to have the composition $O + 1{\cdot}0985$ Cu adopted for the calculation of the correction for 1 grm. of oxygen, because we may well presume that the value of a given quantum of oxide of copper, which contains a small excess of metal (over and above that corresponding to CuO), is very nearly the same as if the surplus copper were present as a mere admixture of reduced metal. If we are right so far, then we have for the volume of O + n Cu grm. of *this* kind of oxide the equation $0{\cdot}11492\,n\,Cu + x = 0{\cdot}18926\,(n\,Cu + 0)$, where x stands for the volume of $O = 16$ grm. of oxygen. For the volume of 1 grm. of oxygen we have $\frac{x}{16} = {\cdot}1628 + {\cdot}1893\,n$,

and for the weight of air displaced by 1 grm. of oxygen at 15° and 760 mm., the same × 1·2267 mgm. Hence, by computation—

For n =	*Weight of air displaced by* 1 *grm. of oxygen.*
1	·4319 mgm.
1·0985	·4553 mgm.
1·2585	·4925 mgm.

We have reason to assume that 1·258 was about the maximum value which n ever assumed in the course of our syntheses, and yet the corresponding air displacement (·4925) is still far below the value ·95 found by direct experiment. The difference, great as it is, must be charged against the absorbed gases.

The S of the syntheses, however, is liable to an additional correction, because in these the metallic copper, before being weighed, was exposed to a long-continued current of dry air, from which it must be presumed to have taken up, however small, a quantity of oxygen chemically. For a guess at the probable magnitude of this error, we re-heated the metal obtained in Exp. I. in hydrogen, and next re-weighed it *in vacuo.* We then allowed 4·5 litres of dry air to pass over it, and weighed it in an air-vacuum. As a last step, the vacuum was undone by admitting hydrogen, this hydrogen pumped out, and the metal weighed in a hydrogen-vacuum. We found, for the *weight of the tube and contents*—

(1) Pure copper in hydrogen-vacuum, ...	tare + 8·0012 grm.
(2) Slightly oxidised copper in an air-vacuum,	tare + 8·0156 grm.
(3) The same in a hydrogen-vacuum, ...	tare + 8·0153 grm.

From (1) and the mean of (2) and (3) we have weight of oxygen absorbed as $Cu_2O = 14·25$ mgm.

To utilise the present experiment as a means for correcting the oxygen-weights found in the syntheses, let us assume that the metallic copper obtained in the first test-experiment, after having been weighed (as pure Cu) *in vacuo*, had been allowed to combine with 14·25 mgm. of oxygen before being weighed in air. The uncorrected oxygen-weight then would have been 21·5128 — ·01425 grm., but the true value S_0 would have been the same as reported. Hence (substituting the means of the two values S_0 and S for those found in Exp. I.), we have,

Faulty oxygen-weight, S = ...	21·06815 grm.
True oxygen-weight, S_0 = ...	21·10245 grm.
Hence, $S_0 - S$ = ...	34·30 mgm.

—and, consequently, for the correction per S = 1 grm. the value + 1·6281 mgm. From our notes concerning the volumes of air which, in the syntheses, were passed over the metal to be weighed, we conclude that in these the weight of oxygen taken up by unit of copper was less than it was in the test-experiment; we, therefore, now proceed to correct the data afforded by the sum of the syntheses on the basis of three successive assumptions.

I.—*The oxygen taken up amounted to* 14·25 *mgm. per* S = 21·068 *grm.*

	Oxygen.	*Water.*	*Hydrogen.*
Uncorrected numbers, as above,	1	1·125369	
Corrections, ...	+ ·001628	+ ·001382	
Corrected numbers, ...	1·001628	1·126751	·125123
	Or, reducing to $S_0 = 1$,		
	1	...	·124920

whence $H_0 = 0·99936$.

II.—*The oxygen taken up amounted to* 7·0 *mgm. per* S = 21·068 *grm.*

	Oxygen.		*Hydrogen.*
Corrected numbers, ...	1	...	·125309

whence $H_0 = 1·00247$.

III.—*There was no oxygen taken up at all.*

	Oxygen.		*Hydrogen.*
Corrected numbers, ...	1	...	·125681

whence $H_0 = 1·00545$.

If our guess at the correction for the sulphurous acid be correct, each of the three numbers for H_0 must be diminished by ·0014, but *this* correction, under the circumstances, is not worth applying.

We will now pass to our

SECOND AND FINAL SERIES OF SYNTHESES,

which we carried out after we had learned to prepare absolutely oxygen-free hydrogen: by passing the gas (after its deoxidation by red-hot metallic copper) over fused caustic potash, followed by

phosphoric anhydride (instead of over vitriol) for its dehydration.* Let us state, at once, that the P_2O_5-tubes never gained weight appreciably, so that, as we now know, they might have been dispensed with.

While preparing for these final experiments, we invented an easy method for obtaining a very perfect vacuum, by means of two ordinary pumps. An ordinary syringe, provided with a solid piston and a two-way cock at the end of the barrel, was combined with a large bottle in which a fair ordinary vacuum was maintained by a second air-pump, in such a manner that the waste air from the syringe, instead of being sent into the atmosphere, was discharged into the vacuum bottle. By proceeding in this manner we easily succeeded in producing a vacuum of less than 1 mm.; and yet we soon came to give up this refinement, because it turned out that the pump proper, when in good condition, exhausted to about 3 mm. easily, and this sufficed for our purpose, because we took care to weigh both the metallic copper and the oxide of copper in a hydrogen-vacuum, besides measuring the pressures of our *vacua* by means of a specially constructed mercury-gauge, shown at Fig. 8. (A rather large syphon-manometer, the vacuum-limb of which was provided with a very good Greiner and Friedrichs' stopcock, while the open limb communicated with a mercury-reservoir. As the tube was about a centimetre wide inside, it was easy to fill the instrument with mercury without introducing more than a trace of air, and the influence of this was reduced to a minimum by expanding the vacuum into the largest available space.) The construction of the reduction-tube is shown by Fig. 9, for the explanation of which it suffices to say that, during use, the joint at C was made tight by means of a close bandage of warm non-vulcanised india-rubber, secured by double wires both at the left and at the right of C, and that during the evacuations, the end H was closed with an india-rubber cap as shown in the figure. J is a piece of red india-rubber tubing, K a rounded glass rod, L a glass cap fitting over the india-rubber. The india-rubber tube is closed at its lower end by means of copper wire, M. For the tightening of the india-rubber tube at J, two pieces of copper

* The corresponding U-tubes were prepared in this way:—A quantity of asbestos was dehydrated by heating, then shaken in a bottle with an abundant supply of the anhydride, and quickly transferred to the U-tube after its bend had been about half-filled with the reagent.

wire of exactly the same weight were provided, one served in the first exhaustion, the other in the second. The joints were found to hold very tight, only in the first rehearsal the rim of the reduction-tube cracked in consequence of the belt of the stopcock part being pressed against it by the atmosphere. This, however, in subsequent experiments, was easily avoided by placing a few small bits of card-paper between the two glass surfaces. The tare for the reduction-tube was made out of a piece of combustion-tubing of the same width as that for the working-tube. This tare-tube was simply drawn out and closed at both ends, but its outer volume was so adjusted that it displaced exactly the same weight of water as the working-tube did, with its cap and stop-cock on.

We might have stated before that there is a stopper of asbestos at F, which was introduced for the first experiment, and never taken out.

In a synthesis, the first step was to charge the reduction-tube with (about 115 grm. of) oxide of copper, and, after having attached the stop-cock, to heat it in a magnesia-bath in a current of dry air (about 4·5 litres). This, as a rule, was done on the day preceding the experiment. The tube was left overnight with the cock closed and the cap wired on, and, on the following morning, it was exhausted, with the pump or combination of pumps. Hydrogen was then admitted into it, and the tube allowed to stand beside the balance while the other apparatus was being weighed. It was next exhausted as completely as possible (the vacuum-meter being read this time) and weighed against its tare. After the attainment of constancy of weight, the tube was left suspended for at least ten minutes, to make sure that no air leaked in. From the beginning of the first to the end of the last weighing, the india-rubber joint at C was kept covered over with a piece of very thin silver foil, to prevent change of weight as far as possible. During the progress of a reduction the parts of the tube which were not meant to be heated, were protected by suitable asbestos anhydride screens.

The water-receptacle was the same as the one which had been used in the first series, except that the U-tubes which followed it were charged, the first with fused caustic potash, the second with phosphoric anhydride. On account of the bulkiness, more than on account of the greater weight, of the reduction-tube, the small balance which we had used in the first series was not conveniently

available this time. We substituted for it an excellent kilo-balance from Oertling, which, like the smaller instrument used before, is provided with a Dittmar's microscopic reading arrangement, and which, even when charged with several hundred grammes on each side, is constant in its indications to within 0·2 to 0·1 mgm.

On account of the introduction of the vacuum-method for the determination of the oxygen, the execution of the experiments was not quite so easy as in the first series, and it took us some time before we became quite familiar with all the manipulations involved. But there would be little use in here giving exact instructions in regard to minor details. Whoever cares to repeat our experiments must go through his own apprenticeship.

In now proceeding to report on the individual experiments, we shall, for brevity's sake, use the following symbols:—

S for the loss of weight suffered by the reduction-tube.

m' and m'' for the tension of the hydrogen, in which the CuO and Cu were weighed, respectively.

S_0 for the corrected weight of the oxygen. As a rule $S_0 = S$.

w_0 for the uncorrected weight of the water obtained in the liquid form.

w' and w'' for the uncorrected weights of the steam condensed in the caustic-potash tube following the water-receptacle, and in the P_2O_5-tube following the KHO-tube, respectively.

t'' for the temperature, and P'' for the pressure, of the atmosphere at the second weighings.

"Air" for the weight of air displaced by the w_0 grm. of liquid water.

W for the total weight of water, uncorrected; W_0 for the same corrected.

h_0 for the weight of hydrogen $= W_0 - S_0$.

H for $8 \times (h_0 : S_0)$; that is to say, the atomic weight of hydrogen, referred to $O = 16$.

Experiment (1). $S = 17{\cdot}0530$; $m' = 7$ mm.; $m'' = 5$ mm.; hence $S_0 = S$. $w_0 = 19{\cdot}0367$. $w' = {\cdot}1457$. $w'' =$ nil. Hence $W = 19{\cdot}1824$ $t'' = 11{\cdot}75°$; $P'' = 748{\cdot}9$ mm. Hence, air displaced by the w_0 grammes of liquid water $= 23{\cdot}3$ mgm; and $W_0 = 19{\cdot}2057$, and $h_0 : S_0 = {\cdot}126\ 236$, and $H = 1{\cdot}009\ 89$.

There was a little difficulty in getting the last drop of water out of the drawn-out end of the reduction-tube into the water-flask; otherwise the experiment proceeded quite normally.

Experiment (2). In this experiment everything proceeded quite normally except that, at the end, when the reduction-tube and water-flask were detached from each other, a minute drop of water was seen adhering to the end of the neck of the former. Rather than simply neglect this water, or lose the experiment, we produced as good an imitation as we could of the droplet at the end of a tared glass tube similar to the neck referred to, and weighed it. It amounted to 4·3 mgm. Allowing this as a correction for the water-weight directly found we had :—$S = 17{\cdot}3342$; $m' = 3{\cdot}5$ mm.; $m'' = 5{\cdot}0$ mm. $S_0 = S$. $w_0 = 19{\cdot}4403$; $w' = {\cdot}0568$; $w'' = 0$. Hence $W = 19{\cdot}4971$. $t'' = 11{\cdot}5°$; $P'' = 756{\cdot}6$ mm. Hence, air displaced = 24·0 mgm, and $W_0 = 19{\cdot}5211$, whence $h_0 : S_0 = {\cdot}126\ 161$, and $H = 1{\cdot}00929$.

If we neglect the drop of water lost we have $h_0 : S_0 = {\cdot}125912$, and $H = 1{\cdot}00731$.

Experiment (3). $S = 17{\cdot}2882$; $m' = 6{\cdot}0$; $m'' = 6{\cdot}0$ mm. Hence $S_0 = S$. $w_0 = 19{\cdot}3892$; $w' = {\cdot}0544$; $w'' = {\cdot}0001$. $W = 19{\cdot}4436$. $t'' = 13°$; $P'' = 748{\cdot}9$. Hence air = 23·6, and $W_0 = 19{\cdot}4672$, whence $h_0 : S_0 = {\cdot}126040$, and $H = 1{\cdot}00832$.

Experiment (4) was lost through the pressure of the atmosphere driving the stopcock part of the reduction-tube against the receptacle of the oxide and causing it to crack.

Experiment (5). In this experiment the air-pump failed to work properly, so that the pressure in the reduction-tube could not be reduced to less than 27 mm. Otherwise everything went on all right.

$S = 20{\cdot}3540$; $m' = m'' = 27{\cdot}0$ mm. Hence $S_0 = S$. $w_0 = 22{\cdot}8360$; $w' = {\cdot}0630$; $w'' = {\cdot}0002$. Hence $W = 22{\cdot}8992$. $t'' = 13°$; $P'' = 754{\cdot}0$. Hence air = 28·0, whence $W_0 = 22{\cdot}9272$, and $h_0 : S_0 = {\cdot}126422$, and $H = 1{\cdot}01138$.

Experiment (6). In this experiment the air-pump again failed to work satisfactorily, and, as a consequence, the two readings of the manometer were inconveniently high, and, what is worse, different from one another. The experiment otherwise proceeded all right.

$S = 20{\cdot}4418$; $m' = 15$; $m'' = 40$ mm.; corresponding correction $= +{\cdot}26$ mgm. Hence $S_0 = 20{\cdot}4421$. $w_0 = 22{\cdot}9232$; $w' = {\cdot}0565$; w''

$= \cdot 0002$. Hence $W = 22 \cdot 9799$. $t'' = 14 \cdot 8°$; $P'' = 758 \cdot 6$. Hence air $= 28 \cdot 1$; $W_0 = 23 \cdot 0080$; $h_0 : S_0 = \cdot 125521$; and $H = 1 \cdot 00417$.

Experiment (7). $S = 20 \cdot 8639$; $m' = m'' = 18$ mm. Hence $S_0 = S$. $w_0 = 23 \cdot 4059$; $w' = \cdot 0608$; $w'' = 0$; $W = 23 \cdot 4667$. $t'' = 15 \cdot 3°$; $P'' = 751 \cdot 7$. Hence air $= 28 \cdot 4$; and $W_0 = 23 \cdot 4951$; $h_0 : S_0 = \cdot 126112$; and $H = 1 \cdot 00890$.

Experiment (8). In this experiment the reduction-tube cracked just before its second exhaustion, hence the metallic copper had to be weighed in hydrogen at the pressure of the atmosphere. We had: $S = 20 \cdot 9152$; $m' = 18$ mm.; $m'' = 752 \cdot 0$; calculated correction $= + 7 \cdot 4$ mgm. Hence $S_0 = 20 \cdot 9226$. $w_0 = 23 \cdot 4745$; $w' = \cdot 0578$; $w'' = \cdot 0005$. Hence $W = 23 \cdot 5328$. $t'' = 15 \cdot 5°$; $P'' = 752 \cdot 0$. Hence air $= 28 \cdot 4$, whence $W_0 = 23 \cdot 5612$; $h_0 : S_0 = \cdot 126112$; and $H = 1 \cdot 00890$.

Experiment (9). $S = 21 \cdot 0957$; $m' = m'' = 3$ mm. Hence $S_0 = S$; $w_0 = 23 \cdot 6543$; $w' = \cdot 0714$; $w'' = 0$, whence $W = 23 \cdot 7257$. $t'' = 15 \cdot 5°$; $P'' = 747 \cdot 4$. Hence air $= 28 \cdot 5$; $W_0 = 23 \cdot 7542$; $h_0 : S_0 = \cdot 126021$; and $H = 1 \cdot 00817$.

Experiment (10). $S = 21 \cdot 8994$; $m' = m'' = 2$ mm. Hence $S_0 = S$; $w_0 = 24 \cdot 5870$; $w' = \cdot 0400$; $w'' = 0$. Hence $W = 24 \cdot 6270$. $t'' = 15°$; $P'' = 752 \cdot 0$, whence air $= 29 \cdot 8$; $W_0 = 24 \cdot 6568$; $h_0 : S_0 = \cdot 125912$; and $H = 1 \cdot 00730$.

Experiment (11). $S = 21 \cdot 8593$; $m' = 1 \cdot 5$; $m'' = 1.0$ mm. Hence $S_0 = S$. $w_0 = 24 \cdot 5407$; $w' = \cdot 0474$; $w'' = 0$; $W = 24 \cdot 5881$. $t'' = 15 \cdot 0°$; $P'' = 751 \cdot 5$. Hence air $= 29 \cdot 8$; $W_0 = 24 \cdot 6179$; $h_0 : S_0 = \cdot 126198$; and $H = 1 \cdot 00959$.

Experiment (12). $S = 21 \cdot 8499$; $m' = 2 \cdot 5$; $m'' = 2 \cdot 0$ mm. Hence $S_0 = S$. $w_0 = 24 \cdot 5067$; $w' = \cdot 0654$; $w'' = - \cdot 0001$; $W = 24 \cdot 5721$. $t'' = 15 \cdot 75°$; $P'' = 761 \cdot 4$. Hence air $= 30 \cdot 0$; $W_0 = 24 \cdot 6021$; $h_0 : S_0 = \cdot 125959$; and $H = 1 \cdot 00768$.

Experiment (13). $S = 21 \cdot 5788$; $m' = 1 \cdot 7$; $m'' = 2 \cdot 0$ mm. Hence $S_0 = S$. $w_0 = 24 \cdot 2118$; $w' = \cdot 0631$; $w'' = 0$; $W = 24 \cdot 2749$. $t'' = 14 \cdot 9°$; $P'' = 761 \cdot 1$. Hence air $= 29 \cdot 8$; $W_0 = 24 \cdot 3047$; $h_0 : S_0 = \cdot 126323$; and $H = 1 \cdot 01059$.

Experiment (14). $S = 20\cdot9709$; $m' = 3\cdot5$; $m'' = 3\cdot7$ mm. Hence $S_0 = S$. $w_0 = 23\cdot5422$; $w' = \cdot0461$; $w'' = -\cdot0001$; $W = 23\cdot5883$. $t'' = 15\cdot2°$; $P'' = 760\cdot6$. Hence air $= 28\cdot9$; $W_0 = 23\cdot6172$; $h_0 : S_0 = \cdot126189$; and $H = 1\cdot00951$.

The above report includes all the experiments that we made, whether successes or the reverse. On the other hand, we have to confess that the displacement of the reduction-tube was not in all cases exactly equal to that of its tare. Originally this was the case to within less than 0·4 c.c., but the reduction-tube had to be renewed twice, and the two new tubes were simply made on the model of the original one, and then weighed against the original tare without readjustment of the latter. Hence, before going any further, we had better calculate the maximum uncertainty which our oxygen weights are infected with on this account. The tare apparatus displaced 167·5 grammes of water, and as we took great care to make the new reduction-tube as nearly as possible identical with the original one, it is almost impossible to assume that their outer volumes differed by more than, let us say, one-fifth of the value, or by 33·5 c.c. We will adopt this number as representing a limit-value for the unknown difference. Now, supposing the temperature and pressure of the atmosphere in the morning, when the first weighings were made, were t'^{0} and P' mm., and the corresponding values at the second weighings were $t'' = t' + \vartheta$ and $P'' = P' + p$ mm. (The latter values are given above, the former are before us in our Journal, but we do not deem it necessary to transcribe them here.) Then we have for the greatest possible value of the error in a given experiment the approximate expression $\pm 33\cdot5 \times \delta \left(\frac{p}{P} - \frac{\vartheta}{T}\right)$, where δ stands for the weight of 1 c.c. of air in milligrammes as it was in the morning, and T for 273 + the temperature as it was in the evening. For the present purpose δ may be put down at the constant value of 1·227, and P at 760 mm. We have calculated the corrections for the several experiments recorded, and found that the correction attains its maximum in the case of experiment (5) for which it is $= -\cdot49$ mgm.; next after it comes experiment (9) with $-\cdot19$; then experiment (6) with $-\cdot16$. For all the rest, the values found were considerably less. Hence this error may be neglected. In the following table the first column gives the number of the experiment; the second, the values $h_0 : S_0$ found; the third, the value x calculated

therefrom for the atomic weight of hydrogen; the fourth, as "residuals" the differences $x = x_0$, where x_0 is the adopted "mean value":—

Summary of Results.

No.		$h_0 : S_0$.		$x = 8 (h_0 : S_0)$.		*Residuals.* $x_0 = 1·00913$.
6	...	·125521	...	(1·00417)	...	− ·00496
10	...	·125912	...	1·00730	...	− ·00183
12	...	·125959	...	1·00768	...	− ·00145
9	...	·126021	...	1·00817	...	− ·00096
3	...	·126040	...	1·00832	...	− ·00081
7	...	·126112	...	1·00890	...	− ·00023
8	...	·126112	...	(1·00890)	...	− ·00023
2	...	·126161	...	(1·00929)	...	+ ·00016
14	...	·126189	...	1·00951	..	+ ·00038
11	...	·126198	...	1·00959	...	+ ·00046
1	...	·126236	...	1·00989	...	+ ·00076
13	...	·126323	...	1·01059	...	+ ·00146
5	...	·126422	...	1·01138	...	+ ·00225

Mean of the unbracketed Nos. (10 experiments),... ... ... 1·009133;

$r = \pm ·00088$, $r_0 = \pm ·00029$.

The bracketed values, x, are excluded on account of the irregularities in the respective experiments, referred to in the context. For the remaining ten, the mean, the probable error, "r," of a single experiment, and the probable error, "r_0," of the mean, are given at the foot of the table. In accordance with the laws of probability, five of the residuals are less, and five are greater than, ·00088. For a guess at the probable uncertainty of the mean result, let us take the mean of (1) the five lowest and (2) the five highest values of x, and divide the difference of the two means by 2. The former mean is 1·00807, the latter is 1·01019; half the difference of the two is ·00106, or 3·6 times the probable error of the mean, which again falls in fairly well with the law of frequency of error.

Perhaps we had no right to exclude experiment No. (6).

If we allow it to vote, the residuals and probable errors stand thus:—

No.		*Residual* = x − 1·00868.		
6	...	− ·00451	...	Mean = 1·00868
10	...	− ·00138		
9	...	− ·00051		
12	...	− ·00100	...	r = $\pm$ ·00131
3	...	− ·00036	...	r_0 = $\pm$ ·00039
7	...	+ ·00022		
2	...	+ ·00083		
14	...	+ ·00091		
11	...	+ ·00121		
1	...	+ ·00191		
13	...	+ ·00270		

Were we asked to name those of our experiments in which we have most confidence ourselves, we should select Nos. 7, 9, 10, 11, 12, 13, 14, because these proceeded with the highest degree of regularity. The mean of these seven experiments, the residuals and the values, r, are as given in the following table:—

No.		*Residual* = x − 1·00882.		
10	...	− ·00152		
12	...	− ·00114		
9	...	− ·00065	...	Mean = 1·00882
7	...	+ ·00008		
14	...	+ ·00069	...	r = $\pm$ ·00079
11	...	+ ·00077		
13	...	+ ·00177	...	r_0 = $\pm$ ·00030

Which of the three means shall we adopt? If there were any considerable difference between them, we should probably say: "the mean of the 11." But the deviations of the three results from one another are only slight, and for this reason we consider ourselves justified in adopting the mean of what we deem to be the seven best experiments as being in all probability the closest approximation to the truth. But, in any case, one correction still remains to be made; we refer to the occluded hydrogen in the metallic copper produced. In the course of our first series, we occasionally determined the occluded hydrogen quantitatively, and arrived at the conclusion that it might safely be neglected. This

is still our opinion as far as that series is concerned, but the present, second, series affords a sufficient degree of constancy in the results to justify its application. Unfortunately, however, we arrived at this opinion only after all the work had been completed. The only thing we can do in the circumstances, is to try and correct our present results by the occluded hydrogen determinations made in connection with the first series.

As a basis for our calculation, we will adopt four of the determinations quoted on page 23. For these, the uncorrected oxygen weights in grammes, and the weights of water obtained from the occluded hydrogen in milligrammes, were as follow:—

Page of Journal.		S.		Water from occluded Hydrogen.
75	...	10·37	...	3·7
77	...	10·53	...	2·7
79	...	10·42	...	2·1
89	...	15·46	...	7·2
		46·78 grms.		15·7 mgms.

Take ϵ as a symbol for the weight of occluded hydrogen per unit-weight of oxygen found, and adopting $O = 16$ as the standard for atomic weights, the correction to be applied to the uncorrected value of H, is $- 9\epsilon$. The value of 9ϵ, as calculated from the above numbers, is = ·0003356, hence we have for the corrected mean of—

	The 10 unbracketed Experiments.		The same and No. (6).		The 7 best Experiments.
H = ...	1·00879	...	1·00834	...	1·00848

The fifth decimal, of course, is of no value whatever; we therefore adopt

H = 1·0085 (O = 16), or O = 15·866, or say 15·87 (H = 1)

as the net result of our work.

The liquid water produced in the first, and also that produced in the second, series had been carefully collected and preserved in glass-stoppered bottles, and at the end of all the work, we examined both for all the impurities that could reasonably be presumed to be present; but we obtained negative results in all cases. Very

delicate litmus-paper remained unchanged, no sulphurous acid could be detected by permanganate, no nitrous acid by Gries' re-agent, no ammonia by Nessler's, no metals by sulphuretted hydrogen. To test for nitric acid 10 c.c. of each water were alkalised by addition of a granule of carbonate of soda, and the solution was evaporated to about 0·5 c.c.; oil of vitriol was now added and ferrous sulphate poured on the top of the mixture as soon as it had cooled down sufficiently. There was no coloration even after long standing. To test for sulphuric acid, 5 c.c. of each of the waters was mixed with a drop of chloride of barium, and allowed to stand over night. No trace of a precipitate could be seen, even in the case of the water from the first series, which rather surprised us. We therefore tried to determine the least quantity of SO_3, which the test would have revealed. A standard sulphuric acid, containing 40 grm. of SO_3 per litre, was diluted with water to 10,000 times its volume, and 5 c.c. of the dilute liquid, containing 0·02 mgm. of SO_3, tested with chloride of barium. After 10 minutes a distinct opalescence was seen, but this did not increase on standing, and we felt convinced that an appreciably less quantity could not have been detected. To this extent therefore our water from the first series may be contaminated with sulphuric acid.

Being well aware that ours is not by any means the first attempt since the days of Erdmann and Marchand, to fix, as far as possible, the ratio H : O, we will now proceed to a brief review of the results of our predecessors.

STAS (Aronstein's translation of his memoir, pp. 57 and 58).—As the mean of 9 experiments which agree almost absolutely with one another, Stas finds that 1 gramme of silver precipitates ·49597 grm. of sal-ammoniac. Hence taking $Ag = 107{\cdot}93$, $Cl = 35{\cdot}454$, and $N = 14{\cdot}046$, we have $NH_4Cl = 53{\cdot}530$, whence $NH_4 = 18{\cdot}076$, and $H = 1{\cdot}0075$, which number, considering that it is burdened with the errors of four experimentally determined constants, agrees wonderfully well with our own. But after all, even Stas' atomic weights cannot be presumed to be free of error, and we need only assume that while his NH_4Cl is by 0·01 too high, his Cl and N are each by 0·01 too low, to bring his value for $4 \times H$ down to 4 exactly. So the agreement, perhaps, is only accidental.

Cooke and Richards (*American Chemical Journal*) vol. 10, pp. 81-110; same journal, same vol., pp. 191-196.—Abstract *Chemical Society's Journal*, "Abstracts," year 1888, p. 647; and *ibid.*, p. 910). C. and R. weighed their hydrogen directly in the Regnault fashion; but it is questionable if they gained much by doing so. Even if the hydrogen is absolutely pure, it is questionable whether the weighing of it, as a voluminous gas, affords a higher degree of exactitude than the indirect mode of taking the difference between the weight of the water and the weight of the oxygen, and, if it is contaminated with nitrogen, the indirect method is positively the more exact of the two. C. and R.'s hydrogen, it appears, was dried with oil of vitriol and phosphoric anhydride used together; hence their gas was probably contaminated with sulphurous acid. In their original determinations, they also neglected to allow for the expansion which their hydrogen-globe suffered, when, after having been tared (against a tare-flask of constant displacement) in an exhausted condition, it was filled with hydrogen of the pressure of the atmosphere. After this oversight had been pointed out to them by Lord Rayleigh, they corrected their original results, and found, finally, $H = 1 \cdot 00825$, which, as we see, comes close to our own number.

W. A. Noyes (*American Chemical Journal*, vol. 11, pp. 155-161; abstract in the *Berichte der deutschen chemischen Ges.; Referate*, year 1889, p. 475).—An apparatus constructed entirely of glass in such a manner that the oxide of copper can be reduced, and the water weighed, within it, is (1) evacuated and weighed. It is then made to communicate with a source of pure hydrogen, the oxide of copper heated, and the water made to condense in the part provided for the purpose, care being taken to keep the oxide of copper slightly in excess. The apparatus is then closed, and weighed. (3) The water is removed by heating the apparatus and sucking out the vapour of water by means of a mercurial air-pump. This being accomplished, the apparatus is weighed a third time. Taking W', W'', and W''' as representing the three weights, we have, for the hydrogen used, $W'' - W'$; and for the water formed, $W'' - W'''$. Six determinations gave (for $H = 1$), $O = 15 \cdot 905$ to $15 \cdot 876$; mean $= 15 \cdot 886 \pm \cdot 0028$. Or, for $O = 16$, $H = 1 \cdot 00717$.

LORD RAYLEIGH (*Proceedings Royal Society*, vol. 45, p. 425).—Two glass-globes of about 1800 c.c. capacity each are charged, one with hydrogen, and the other with oxygen, of about one atmosphere's pressure, and they are then tared, each against a tare-globe of exactly the same displacement. Suitable quantities of the two gases are then extracted by means of a mercurial air-pump (about 0·1 grm. of hydrogen and a slight excess of oxygen), and mixed together in a mercurial gas-holder. The large volume of fulminating gas thus produced is exploded, in instalments, in the same eudiometer, and, in the ultimate residue obtained, the oxygen is determined volumetrically, to be reduced to weight by calculation. The weights of oxygen and hydrogen extracted from the globes are determined by weighing the globes after extraction of the gases, the shrinkage owing to the diminution suffered by the internal pressure being allowed for. In this manner all the data for calculating the ratio O : H are procured. Five experiments gave for its value 15·92 to 15·98, without allowing for shrinkage. The correction for it lowers the mean (15·95) by four parts in a thousand, and brings it down to 15·89, corresponding to H = 1·00692, for O = 16. In connection with research by Rayleigh, it is important to notice that he used no oil of vitriol, but only fused caustic potash combinedly with phosphoric anhydride for drying his hydrogen, and took all imaginable precautions for avoiding contamination of his gas with atmospheric air.

Last, not least, we come to that admirable research which E. H. Keiser published in the *American Chemical Journal*, vol. 10, pp. 249—261.* Of all the methods used so far for the determination of the gravimetric composition of water, Keiser's impresses me as being the one which offers the surest guarantee for a

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* At the time when we started our investigation, Keiser's experiments were known to me only by his preliminary notice in the *Ber. der deutschen chem. Ges.*, in which he gives three determinations of the ratio O : H, the mean of which is 15·872. My impression at the time was, that in all probability, Keiser's palladium hydride contained water, which, by being weighed as hydrogen, made his value for O : H too low. His full memoir I saw for the first time when engaged in the compilation of this paper. If it had come to me in time, the present research would perhaps never have lost its original, purely critical, character.—W.D.

correct result. Keiser's great hit is that he converts his hydrogen into hydride of palladium and weighs it in this compact form. The condensed hydrogen is re-expelled by heat, burned with oxide of copper, and the water collected and weighed, the displaced air being of course allowed for. The palladium (which, in Keiser's case, amounted to about 150 grammes) is contained in a glass-tube provided with a soldered-on glass stopcock, constructed like the one characteristic of Lunge's nitrometer, so that we can either send off the liberated hydrogen by the horizontal boring or shut off the palladium-tube, and sweep the rest of the apparatus with a current of nitrogen or oxygen. The first step is to heat the palladium *in vacuo* to 250° for about 15 minutes. The palladium-tube is then made to communicate with an apparatus discharging hydrogen purified; finally, by means of a column of red-hot metallic copper, followed by an U-tube charged with phosphoric anhydride. The hydrogen proper combines with the palladium, the traces of nitrogen with which the gas was contaminated remain outside the palladium, and are removed by means of the air-pump. The exhausted tube is weighed against a tare-tube of the same displacement. It is then connected with the oxide of copper tube, the latter heated to redness, and the apparatus next filled with nitrogen to preclude the possibility of an explosion. After this preliminary operation, the hydrogen is being gradually liberated, and converted into water. When the hydrogen is expelled as far as necessary, the palladium-tube is again shut off, and the hydrogen, which stagnates in the rest of the apparatus, swept out by means of a current of nitrogen. At the end the oxygen is turned on and kept going until the reduced copper is completely re-oxidised and its occluded hydrogen recovered as water. Ten experiments gave for the ratio O : H values varying from 15·943 to 15·958; mean = 15·9492, or, for O = 16, H = 1·00318, which is by ·00529 less than our own adopted value! I have tried hard to explain this not inconsiderable difference, and at last come to conceive the following hypothesis, which I give for what it may be worth. Keiser informs us that he *purified* his nitrogen by passing it successively over oil of vitriol, red-hot metallic copper, and phosphoric anhydride; but he does not tell us how he prepared it. There can, however, be little doubt that he prepared it as other people would have done, namely by passing a current of purified air over red-hot copper previously reduced in hydrogen. That a man like Keiser should have forgotten to clear out all the *free*

hydrogen left in his copper-tube before using it for de-oxygenating his air, is not to be presumed, but, in whatever way he may have done this, the reduced metallic copper was bound to contain occluded hydrogen, and it is not absurd to presume that this occluded hydrogen *did not all assume the form of water* when the air passed over it at a red heat. His nitrogen, as it came out of the gasholder, may have been contaminated with a trace of hydrogen, and, as a necessary consequence, his number for the weight of water corresponding to 2 parts of hydrogen may be too high. Supposing, for a moment, that Dittmar and Henderson's value for O : H is the true value, then the 58·86263 grm. of water which Keiser produced in his ten syntheses (out of 6·5588 grm. of palladium-hydrogen), included ·2742 grm. of water from out of the hydrogen of his nitrogen gas; or, in other words, Keiser produced 27·4 mgm. of adventitious water per synthesis, because the nitrogen gas he used contained (say) 3 mgm. = about ·03 litre of hydrogen. I find it difficult myself to believe that it contained so much. Besides, my hypothesis cuts two ways: If nitrogen produced from reduced copper and air is liable to be contaminated with hydrogen, then our own determinations of the occluded hydrogen in certain of the lots of reduced copper which we produced in our first series falls short of the truth; and if they do, our "H = 1·0085" is liable to a negative correction. Being an incorrigible "Proutian," I do not give up the hope that the true number may be 1·0000 after all.

To sum up: The weight of hydrogen which unites with 8 grammes of oxygen into water is, according to—

Dumas' experiments, as corrected by us—

(a) Assuming his water weights are the true weights,	0·989
(b) Assuming that he forgot to reduce his water weights to the vacuum, ...	1·0002
Erdmann and Marchand's, as corrected by us, ...	1·006

(These three numbers, of course, are mere guesses, and must be taken for what they are worth).

Stas, ...	1·0075
Cooke and Richards, ...	1·0083
W. A. Noyes, ...	1·0072
Lord Rayleigh, ...	1·0069
Dittmar and Henderson, the seven best experiments, ...	1·0085

These five independent investigations might be said to settle the question as far as it is possible in the present state of quantitative chemistry to settle it at all, if it were not for—

E. H. Keiser, who finds ... ... 1·0032

and it is impossible to pass over *his* research !

Prof. DITTMAR'S LABORATORY,
ANDERSON'S COLLEGE,
GLASGOW, *August*, 1890.

V.—*Chronological Tables of Scientific Men, showing the Names of the more distinguished Anatomists and Physiologists, and their Contemporaries.* By JOHN G. M'KENDRICK, M.D., LL.D., F.R.SS.L. & E., Professor of Physiology in the University of Glasgow.

[Communicated to the Society, 19th November, 1890.]

THE tables to which these remarks are introductory were first prepared to illustrate a course of lectures on the history of physiological discovery, delivered at the Royal Institution of Great Britain, London, in 1883. Since then they have been amplified by the introduction of new names, and every precaution has been taken to secure accuracy. The tables give (1) the names of the more remarkable anatomists and physiologists; (2) the names of contemporary representatives of other sciences; (3) the names of contemporaries distinguished in philosophy, literature, and art; and (4) a note of one or more events or personages indicating the historical position of any particular decade. The years of birth and death are recorded in all cases where the information was available, and the name of the person is placed in the decade in which it is probable he did the chief work of his life. I have marked the department of science chiefly cultivated by each man by letters such as *A.*, anatomist, *P.*[1], physiologist, &c.; but I have not thought it necessary to indicate the pursuit of the representatives of philosophy, literature, and art. The period embraced is from 1500 to 1860.

Such tables help the imagination in picturing the characteristics of the period in which any notable man of science lived. We see the names of those who at the same time were influencing the minds of men in the realms of philosophy, literature, and art; and we recognise the more important contemporary events which mark the progressive stages of civilisation. Thus we can form some conception of the moral and intellectual atmosphere of the periods when the greatest discoveries in science were made. A short commentary on the tables will show how they may be read.

At the beginning of the sixteenth century, Achillini, sometimes named the second Aristotle, the first anatomist who dissected the human body, was lecturing on anatomy, medicine, and philosophy in Bologna; Linacre was then in practice as a physician in London, and founded the Royal College of Physicians; the learned Erasmus was preparing himself by study in Paris and the Netherlands for his great work; Ariosto was probably writing "Orlando Furioso;" Leonardo da Vinci and Titian were in the height of their fame; and Albert Dürer, after his travels, had settled in his native town of Nuremberg, and was engaged on his sixteen woodcuts on the Apocalypse. Ten years later, Paracelsus, the physician and philosopher, was brooding over the mysteries of alchemy; Sir Thomas More produced "Utopia" in 1516; Luther was giving Biblical lectures in the University of Wittenberg; Michael Angelo was decorating the ceiling of the Sixtine Chapel; Raphael was producing some of his finest works in the splendid period of Leo X.; and Correggio was engaged on frescoes at Parma. This was also the time of Charles V. of Spain; Cardinal Wolsey had reached the summit of his power; and the year 1513 is memorable for the Battle of Flodden Field. From 1520 to 1530, a truer theory of the mechanism of the heavens was shaping itself in the mind of Copernicus; Rabelais was working at "Pantagruel" and "Gargantua;" Holbein was painting altar pieces; Palissy, living in obscurity and poverty, was sacrificing everything in the search after the secret of the white enamel; and the Reformation was then convulsing Germany. During the next twenty years (1530-1550), Vesalius was overturning the erroneous anatomy of Galen, and was laying the foundations of the true science; and other Italian anatomists, whose names are still stamped on the nomenclature of the science, were at work, such as Fallopius, Columbus, and Eustachius; Calvin was then working out his austere but logical system of theology, and religious enthusiasm was excited in the Church of Rome by the fervour of Ignatius Loyola; and John Knox was preparing for the struggle of the Reformation in Scotland. In 1553 appeared the "Restitutio Christianismi" of Servetus, for which he was burnt at the stake. The leading artists were Paul Veronese and Tintoretto; and Camoens was then probably engaged on his famous epic, the "Lusiad." The great events of the period were the Reformation in England, the foundation of the Jesuits, and the Council of Trent.

The next forty years, from 1560 to 1600, witnessed the works of the precursors of Harvey, the discoverer of the circulation of the blood. Fabricius ab Aquapendente, Aranzi, Varolius, Bauhin, and others, investigated the mechanism of the heart and its valves. In particular, about 1580, Caesalpinus, an Italian anatomist, came near the discovery of the circulation; indeed, the honour is claimed for him by his countrymen, and a statue recording the event has been erected to his memory. About this period also, Sanctorius made quantitative examinations of various physiological processes, Gilbert examined electrical phenomena, Snellius in Leyden and Tycho Brahe in Denmark were engaged in their astronomical observations, and Battista Porta was writing on physiognomics and optical phenomena. In the last decade of the century we find Kepler enunciating the laws that regulate planetary motion, and Bacon publishing his first philosophical essays. Giordano Bruno was then promulgating in many countries advanced doctrines, some of which have stood the test of time, but for which he died at the stake. These forty years also witnessed the appearance of the works of George Buchanan, of Tasso, of the "judicious" Hooker, of Sir Walter Raleigh, and, above all, of Spenser. Then were also laid the foundations of English law by Coke, and Cervantes produced Don Quixote, "the child of his wit born in a gaol." Drake sailed round the world in 1577, and the wreck of the Spanish Armada marks 1588.

Contemporary with William Harvey, who taught the new doctrines of the circulation of the blood in 1616, although they were not formally published till 1628, we find Drebbel, the inventor of the compound microscope, Galileo, the physicist and astronomer, Napier, the inventor of logarithms, and Van Helmont, great as a physician, physiologist, and chemist. At that period also, Shakespeare wrote his immortal plays, the first edition appearing in 1623, and then flourished "rare Ben Jonson." The Pilgrim Fathers made their journey to America while doctors were disputing as to those new theories regarding the circulation of the blood. About 1620, Asseli made the important discovery of the lacteal system. Rubens and Vandyck were the artists of the period. "Saintly" George Herbert, the first poet of Anglican theology, was then writing the "Temple" in the parsonage of Bemerton, near Salisbury, and among his friends were Izaak Walton and Francis Bacon. Cardinal Richelieu was then the master of France.

The period between 1630 and 1660 was remarkably rich in great men. Among the anatomists and physiologists we find Sylvius, who worked on the anatomy of the brain, Borelli, who investigated animal locomotion with mathematical precision, Bartholin and Wirsung, who examined the anatomy of glands, Schneider, who described the organs of sense, Pecquet, who discovered the thoracic duct, Willis, who dissected and described the central nervous system, more especially the brain, and Swammerdam, who laid the foundations of accurate visceral anatomy. Contemporary with these was the great Descartes, Otto Guericke, the inventor of the air pump, Horrocks, the astronomer, Pascal, physicist and philosopher, and Huyghæns, well known for researches in the science of optics. In the first part of the period, medicine was represented by Kenelm Digby, and in the latter part by the much greater Sydenham, the father of modern medicine. While these men were engaged in science, Milton was writing his immortal epics, Herrick was singing his lyrics, and Corneille excited the jealousy of Richelieu and the Academy by producing the "Cid." A galaxy of artists existed, composed of Velasquez, Claude Lorraine, Rembrandt, Salvator Rosa, and Murillo. Hobbes was then a power in literature; Jeremy Taylor was the preacher of the age; Molière was acquiring the experience of a travelling actor that enabled him, between 1662 and 1673, to produce his unrivalled comedies. In the political drama we find the signing of the Covenant in Scotland, the trial of Hampden, the civil war, the death of Charles I. in 1649, and the brief protectorate of Cromwell from 1653 to 1658.

Great names also appear in the record of the next forty years, from 1660 to 1700. Boyle investigated the properties of air and of gases, and he led the way to knowledge of the respiratory process. Jollyfe discovered the lymphatics; Malpighi demonstrated under the microscope, to the astonished gaze of Harvey, now an old man, the circulation of the blood in the web of a frog's foot, thus completing the discovery of the circulation; Lower first transfused blood from one living being into another; Ray and Grew laid the foundations of modern natural history; and Leeuwenhoek revealed, by his simple microscope, many of the wonders of the invisible world.

Among these men also, special mention must be made of John Mayow, who first had correct views of the theory of the respiratory

process, and who nearly discovered oxygen. Then also flourished Vieussens, Meibom, Duvernay, Glisson, Ruysch, Peyer, Lancisi, Brunner, Valsalva, Havers, and Bartholin, names well known to the student of human anatomy. Towards the close of the century Parfour du Petit investigated, for the first time, the physiology of the sympathetic system of nerves. This was also the period of Spinoza. Isaac Barrow was the first Lucasian Professor of Mathematics in Cambridge, from 1663 to 1669, but in the latter year, in order to devote himself to the study of theology, he resigned his chair to his illustrious pupil, Isaac Newton, who soon became a star of the first magnitude in the intellectual firmament. Between 1690 and 1716, Leibnitz composed his chief philosophical works, and for thirty years (from 1670 to 1700), he was busy with mathematics, natural science, theology, jurisprudence, economics, and philology. In pure literature we have Bunyan, the inspired dreamer, Bossuet and Massillon, the French preachers, and Racine, the dramatist. Philosophy is represented by Malebranche, and still more by John Locke, whose writings turned the current of philosophic speculation into a new direction. Purcell, the father of English music, then lived, and Christopher Wren was erecting in London some of the buildings that bear the stamp of his architectural genius.

The first half of the eighteenth century shows on its roll the names of Boerhaave, physiologist and physician; Stephen Hales, an English rector at Teddington, in Kent, who, in the quietude of his country parish, first made accurate measurements of the force of the blood in the vessels, and laid the foundations of our present knowledge of the hydrodynamics of the circulation, both in plants and animals; and Haller, the great Swiss physiologist and physician, who carried on with Whytt, a professor in the University of Edinburgh, and the ancestor of the novelist Whyte-Melville, a lively controversy as to the inherent irritability of muscle. Just about the time of this controversy, Scotland was agitated by the events of the '45, Needham was experimenting on spontaneous generation, Trembley was demonstrating the wonderful vitality of the little *Hydra*, Buffon was engaged in his great works on natural history, and Meckel, Zinn, and Camper were busy in anatomical investigations. William Cullen, about the middle of the century, had reached the zenith of his career as a physician in Edinburgh. In the other sciences we find Stephen Gray investigating magnetism. The mathe-

maticians include such names as Jurin, Maclaurin, Bradley, Euler, Daniel Bernoulli, D'Alembert, and Simson, while among the physicists we find Papin, Maskeleyne, Dollond, and Boscovitch. Linnæus was then establishing his great system of classification. It was about this period also that we find the more distinguished members of the remarkable family of the Bernoullis, all more or less remarkable as mathematicians and physicists, including James Bernoulli I., who was born in 1654; Nicholas Bernoulli I., John Bernoulli I., Daniel Bernoulli I., John Bernoulli II., John Bernoulli III., Daniel Bernoulli II., James Bernoulli II., and Christopher Bernoulli, who was born in 1782, and lived well into the present century. Thus the Bernoullis were represented in the world of science for nearly 150 years. It is scarcely necessary to mention the great men who then adorned literature. Defoe, Swift, Addison, Pope, Montesquieu, Voltaire, Metastasio, Fielding, Condillac, Rousseau, Johnson, Smollett, Diderot, and Lessing appear on the roll. The idealistic philosophy of Berkeley appeared at this time, and David Hume, following Locke, resolved philosophy into universal scepticism. Painting is represented by Watteau, Hogarth, Gainsborough, and Reynolds. The fugues of Bach, the oratorios of Handel, and the soft harmonies of Pergolesi were produced about this time. John Wesley was engaged in the great revival of religion now associated with his name, David Garrick was the actor of the day; and while Swedenborg was dreaming his dreams and seeing his visions, Clive was laying the foundations of our Indian Empire, Wolfe was establishing the Dominion of Canada, and the first Pitt, as the Premier of England, was animating and directing the achievements of the British armies in the four quarters of the globe.

The latter half of the century was remarkable for great progress in science. In these years John Hunter and William Hunter, natives of Lanarkshire, pursued their anatomical and physiological studies with great success, and formed those collections which John bequeathed to the Royal College of Surgeons of England, now in Lincoln's Inn Fields, and William to the University of Glasgow, constituting the nucleus of our Hunterian Museum. The Abbe Spallanzani, as an experimental physiologist of the first order, advanced the knowledge of digestion, respiration, and generation; Hewson, a young surgeon in London, examined the properties of chyle, lymph, and blood, and pointed out the true functions of such organs as the lymphatic glands, the spleen, the

thymus, &c.; Unzer took the first steps towards a correct theory of reflex actions; and Erasmus Darwin, while engaged in the arduous duties of the medical profession, wrote "Zöonomia" and other works, which contain the germ of the brilliant generalisations of his immortal grandson, Charles Darwin. This is the period also of the epoch-making investigations of Galvani into the action of electricity on the nerves and muscles of frogs, from which, by the criticisms of Volta and many others, has developed the science of electricity as it now stands. James Watt's invention of the separate condenser of the steam-engine was made in 1769, and Arkwright's spinning "jenny" appeared in the same year. These two machines effected a revolution, and to them may be traced the origin of many of the economic questions of the present day. In these years also Joseph Black discovered carbonic acid, and this discovery, combined with the brilliant investigations of Lavoisier into the effects produced on air by breathing, advanced our knowledge, not only of respiration but of all processes of combustion. Lavoisier pursued his investigations amidst the turmoil of the French Revolution, and at last became one of its victims, after craving a delay of a few days that he might finish his experiments. Hutton may be said to have then started a careful examination of the crust of the earth, Coulomb and Franklin advanced into the realm of electricity, and Biot and De Saussure made many discoveries in physics. Then it was also that Mesmer excited the Parisians by his strange experiments, and started investigations that now and again attract attention under the names of mesmerism, animal magnetism, odylic force, electro-biology, and hypnotism. The historian Gibbon was then at work. Lavater was developing his theories of physiognomy, now almost forgotten, and the University of Glasgow had among its professors Thomas Reid and Adam Smith.

Lamarck was then enunciating his theory of development, which must always be a guide in investigation. The science of chemistry was advanced by Priestley, Scheele, Fourcroy, and Dalton. Modern chemistry may be said to date from the discovery of oxygen by Priestley in 1774, and the atomic theory of Dalton became the key to the chemical constitution of matter. The mathematicians and physicists numbered in their ranks such men as Playfair, Hayley, Laplace, Lagrange, Legendre, Carnot, Fourier, Leslie, Haüy, Rumford, and Ampère. Botany was represented by Joseph Banks and Jussieu, and Cuvier was

the giant among comparative anatomists. Amidst these coruscations of scientific discovery, Goldsmith was writing the "Vicar of Wakefield," Cowper the "Task," Robert Burns was singing his songs in Ayrshire, and Schiller was the poet of Germany. The musicians were Gluck, Mozart, Haydn, Weber, and, chief of all, Beethoven, David was the great painter, and Flaxman and Canova were then at the height of their fame as sculptors. A new departure was taken in philosophy by Kant, the results of which will influence human thought for all time. The brilliant Sheridan was then at his best, and Parliament listened to the eloquence of the second Pitt, of Charles Fox, and Edmund Burke.

Perhaps the most remarkable period of all includes the last ten or fifteen years of the eighteenth and the first ten years of the nineteenth centuries.

The number of distinguished men in all departments about this period is so great that one cannot arrange them into decades, and they can only be viewed as a group. I have sometimes thought that possibly human intellect attained its greatest development about this time, which may be fitly compared too with the most glorious period of ancient Greece. Already I have mentioned a number of names belonging to this time. Then flourished Thomas Young, the discoverer of the key to hieroglyphic inscriptions, the author of the undulatory theory of light, a physiologist who brought his wide knowledge of physics to bear on the investigation of our perceptions of colour, on time in physiological processes, and on the hydraulics of the circulation—a man who was said by Helmholtz to be a hundred years ahead of his contemporaries. This was the period also in which their best work was done by the physicists Gauss, Malus, Arago, Seebeck, Oersted, Peltier, Fresnel, Wollaston, Frauenhofer, Nobili, Ohm, Cagniard de la Tour; and by the chemists Thomas Thomson, Gay Lussac, Brande, Becquerel, Berzelius, and Niepce, one of the pioneers of the modern art of photography. Goethe was then at his best. Hegel, Schelling, and Schopenhauer were framing their systems of philosophy; William Blake was flashing his meteoric genius, the products of which strike one as if they had come from another world; Jeremy Bentham was working out his system of philosophy which culminated in the labours of the two Mills and of Alexander Bain; and Walter Scott was throwing off the products of his genius, learning, and vivid imagination with reckless profusion of brain

power, which brought its own nemesis. This was the time also of the sweet songs of Beranger, of the philosophy and poetry of Coleridge, of the interpretations of nature of Wordsworth, of the passion of Byron, of the intense but dreamy genius of Shelley, and of the finely artistic productions of the immortal Keats. All these gifts were presented to the world during the agonies of the French Revolution and the Wars of Napoleon.

Since 1810 science has made rapid strides. In physiology we have the names of Beaumont, who investigated the processes of digestion in man about 1824; of the brothers Weber, more especially E. H. Weber, who examined muscular action and the circulation; of Marshall Hall, who, following Unzer and Prochaska, established the doctrines of reflex action; of Flourens and Magendie, who were for many years the leading physiologists of France; of Johann Müller of Berlin, whose views as to the specific energy of nerves did much to guide subsequent investigators in the field of the physiology of the organs of sense; of Fechner, who was the pioneer in the investigation of psychophysical phenomena; and, lastly, of Claude Bernard, the great Frenchman, whose discoveries in physiology are too numerous to mention. The anatomists included such men as Krause, Von Baer, Schwann, Schroeder van der Kolk, and John Goodsir. The period about 1840 was particularly fruitful. Then the cell theory was promulgated, and the researches of Du Bois-Reymond into electro-physiology, of Helmholtz into muscular action, nervous action, physiological optics, and physiological acoustics, and of Ludwig into the circulation, then commenced. The names of their scientific contemporaries scarcely require to be mentioned. Chevreul, Faraday, Buckland, Chasles, Struve, Mary Somerville, Cauchy, Babbage, Mitscherlich, Poggendorff, Charles Lyell, William Rowan Hamilton, Listing, John Herschell, Challis, Christison, Mulder, Gassiot, Dumas, Liebig, Mayer, Joule, Léverrier, Adams, Draper, Wheatstone, Andrews, Bunsen, are familiar to every student in science. The leaders in literature and art, during this period, are well known to every one.

A.	Anatomist.	*Th.*	Theologian.
$P._1$	Physiologist.	*Ph.*	Philosopher.
$P._2$	Physician.	*N.*	Naturalist.
$P._3$	Physicist.	*S.*	Surgeon.
C.	Chemist.	*E.*	Engineer.
B.	Botanist.	*CA.*	Comparative Anatomist.
As.	Astronomer.	*G.*	Geologist.
M.	Mathematician.		

NOTE.—For some of the dates in the fourth column I am indebted to Professor Nichol's *Tables of European History, Literature, Science, and Art, from* 200 *to* 1888. 4th ed. J. Maclehose & Sons, Glasgow, 1888.

Period.	Anatomists and Physiologists.	Representatives of Collateral Sciences.	Representatives of Philosophy, Literature, and Art.	Collateral Events.
1500	Achillini, *A.*, 1461-1512.	Linacre, *P.*$_2$, 1460-1524.	Erasmus, 1467-1536. Ariosto, 1474-1533. Leonardo da Vinci, 1452-1519. Titian, 1477-1576. Albert Dürer, 1471-1528.	Julius II., 1503. Colet founds St. Paul's School, 1512.
1510	—	Paracelsus, *P.*$_2$ and *C.*, 1493-1541.	Sir Thomas More's "Utopia" (1516), 1480-1535. Luther, 1483-1546. Michael Angelo, 1475-1564. Raphael, 1483-1520. Correggio, 1494-1534.	Charles V. of Spain. Flodden, 1513. Wolsey, 1471-1530. Tyndale's N. Testament, 1526.
1520	—	Copernicus, *As.*, 1473-1543.	Rabelais, 1495-1553. Holbein, 1497-1543. Palissy, 1499-1589.	Reformation in Germany, 1519-1530. James V. of Scotland, 1528-1542.
1530	Vesalius, *A.*, 1514-1564.	—	Calvin, 1509-1564. Ignatius Loyola, 1491-1556.	Foundation of the Jesuits, 1534. Reformation in England, 1534.
1540	Fallopius, *A.*, 1523-1562. Columbus, *A.*, *d.* 1559.	Ambrose Paré, 1507-1590. (*Ligature of arteries.*)	Ascham, 1515-1568. Tintoretto, 1512-1594.	Council of Trent, 1545-1563.
1550	Eustachius, *A.*, 1520-1574. Dodonée, *P.*$_3$ and *A.*, 1518-1585. Servetus, *Th.* and *P.*$_1$, 1509-1553. (*Discovery of pulmonary circulation.*)	—	John Knox, 1505-1572. Paul Veronese, 1528-1588. Camoens, 1527-1579.	Martyrdom of Servetus, 1553. Elizabeth, 1558-1603.

1560	Fabricius ab Aquapendente, *A.* and $P._1$, 1537-1619. Aranzi, *A.*, 1530-1589.	Libavius, *C.*, *d.* 1618. (*Chemical text-books.*)	George Buchanan, 1506-1582. Palestrina, 1524-1594.	Reformation in Scotland, 1560. Queen Mary Stuart, 1562-1568. London Royal Exchange, 1571.
1570	Varolius, *A.*, *b.* 1543.	Gilbert, $P._3$, 1540-1603. Snellius, $P._3$, 1547-1613. Mercator, *M.*, 1512-1594.	Isaac Casaubon, 1559-1614. Tasso, 1544-1595.	XXXIX. Articles, 1571. Peace of Utrecht, 1579. Drake sails round the world, 1577. Massacre of St. Bartholomew, 1572.
1580	Piccolomini, *A.* and $P._2$, *b.* 1556. Bauhin, *C.* and *B.*, 1550-1624. Alberti, *A.*, about 1581. Donatus, *A.*, about 1588. Cæsalpinus, *A.*, 1519-1603.	Tycho Brahé, *As.*, 1546-1601. Giordano Bruno, *Ph.*, *d.* 1600. Giambatista Porta, *C.*, 1538-1615.	Hooker, 1553-1600. Raleigh, 1552-1618. Spenser, 1553-1599. ("Faerie Queene," 1590.)	Edin. Univ., 1582. The Armada, 1588.
1590	Sanctorius, *C.* and $P._1$, 1561-1636.	Kepler, *As.*, 1571-1630. Bacon, *Ph.*, 1561-1626. (*First ed. of Essays, 1597.*)	Coke, 1550-1634. Cervantes, 1547-1616. ("Don Quixote," 1605.) Marlowe, 1564-1593.	Presby. Church established in Scotland, 1592. Edict of Nantes, 1598. Bodleian Library restored, 1597.
1600	Van der Spiegel, *A.*, 1578-1625.	Drebbel, $P._3$, 1572-1634. (*First compound microscope, 1590.*) Galileo, $P._3$, 1564-1642. (*Invention of thermometer.*)	Shakespeare, 1564-1616. Rubens, 1577-1640.	James I., 1603-1625. Gunpowder Plot, 1605. East India Coy.'s Charter, 1600. Virginia founded, 1607.

Period.	Anatomists and Physiologists.	Representatives of Collateral Sciences.	Representatives of Philosophy, Literature, and Art.	Collateral Events.
1610	William Harvey, *A.* $P._1$, and $P._2$, 1578-1657. (*Circulation of the blood—De Motu Cordis et Sanguinis, 1628.*) Van Helmont, $P._1$ and $P._2$, 1577-1644. (*Beginning of clinical medicine.*) L. G. Hoffman, *A.*, 1572-1648.	Napier, *M.*, 1550-1617. (*Logarithms in 1614.*)	English Bible. Ben Jonson, 1574-1637. Selden, 1584-1654.	The Pilgrim Fathers, 1620. Thirty Years' War, 1618-1648. Authorised version of Bible, 1611. First Water Supply for London, the "New River," 1613.
1620	Riolan, *A.*, about 1626. Asselli, *A.*, about 1622. (*Discovery of lacteals.*)	Gassendi, $P._3$, 1592-1655. Riccioli, $P._3$, 1598-1671.	George Herbert, 1593-1633. Vandyck, 1599-1641. Grotius, 1583-1645.	Foundation of New York, 1624. Richelieu, 1585-1642. First English Newspaper, 1622. First Edition of Shakespeare, 1623. Petition of Right, 1628.
1630	Deleboc, *A.*, 1614-1672. (*Known as Sylvius.*)	Descartes, *Ph.* and *M.*, 1596-1650. Von Guericke, $P._3$, 1602-1686. (*Invention of air pump.*) Stevinus, *C.*, 1633. Horrocks, *As.*, 1619-1641. Wallis, *M.*, 1616-1703. Ashmole, $P._3$, 1617-1692. Kenelm Digby, $P._3$ and *C.*, 1603-1665. Glauber, *C.* and $P._2$, 1603-1668.	Herrick, 1591-1674. Velasquez, 1599-1660. Corneille, 1606-1684.	Covenant in Scotland, 1638. Trial of Hampden, 1637-8. Long Parliament, 1640-1653. French Academy, 1635.

1640	Borelli, *A.* and $P._3$, 1608-1679. T. Bartholin, *A.*, 1616-1680. Wirsung, *A.*, about 1642. Schneider, *A.*, 1610-1680.	Torricelli, $P._3$, 1608-1647 (*Invention of barometer, 1643.*)	Milton, 1608-1674. (First ed. of "Paradise Lost," 1667.) Claude Lorraine, 1608-1682. Rembrandt, 1607-1669. Salvator Rosa, 1615-1673. Murillo, 1618-1682.	Civil War, 1642-1651. Marston Moor, 1644. Foundation of Royal Society, 1645. Confession of Faith, 1648. Execution of the King, 1649. Tasmania discovered, 1642.
1650	Van Horn, *A.*, 1621-1670. Rudbeck, *A.* and $P._1$, about 1650. Pecquet, *A.*, about 1651. (*Discovery of thoracic duct.*) Highmore, *A.* and $P._2$, about 1651. Willis, *A.* and $P._2$, 1622-1675. Swammerdam, *A.*, 1637-1680. Boyle, $P._1$, $P._3$, and *C.*, 1627-1691.	Pascal, $P._3$ and *Ph.*, 1623-1662. Huyghens, *As.*, 1629-1695. Sydenham, $P._2$, 1624-1689.	Hobbes, 1588-1679. Molière, 1622-1673. Jeremy Taylor, 1613-1667. Jan Steen, 1626-1679. Isaac Walton, 1593-1683. ("Compleat Angler," 1653.)	Cromwell Protectorate, 1653-1658. Louis XIV., 1653-1715. Navigation Act, 1651.
1660	Jollyfe, *A.*, *b.* about 1622 (*Discovery of lymphatics.*) Malpighi, $P._1$, 1628-1694. (*Demonstration of circulation under the microscope.*) Lower, *A.* and $P._2$, 1631-1691. (*Transfusion of blood.*) Ray, *N.*, 1628-1704. Grew, *N.* and $P._1$, 1628-1711. M. Hoffmann, *A.*, 1622-1698. Stenon, *A.*, 1638-1687.	James Gregory, $P._2$ and *C.*, 1638-1675. (*Reflecting telescope.*)	Spinoza, *Ph.* 1632-1677. Bunyan, 1628-1688. ("Pilgrim's Progress," 1678.) Bossuet, 1627-1704. Isaac Barrow, *Ph.* and *Th.*, 1630-1677. Samuel Pepys, 1632-1703.	First Standing Army. Plague of London, 1665. Fire of London, 1666. Dutch War, 1667.
1670	Vieussens, *A.* & $P._2$, 1641-1716.	Roemer, $P._3$, 1644-1710.	Dryden, 1631-1700.	Habeas Corpus Act, 1679.

Period.	Anatomists and Physiologists.	Representatives of Collateral Sciences.	Representatives of Philosophy, Literature, and Art.	Collateral Events.
1670	Meibom, *A.*, *b.* 1638. Leeuwenhoek, $P._1$, 1632-1723. Hooke, P_3. and $P._2$, 1635-1702. (*Theory of respiration.*) Mayow, $P._1$ and *C.*, 1645-1679. (*Theory of respiration.*) Duverney, *A.* and $P._1$, *b.* 1647. Bellini, *A.*, *b.* 1643. Glisson, *A.*, 1596-1677.		Racine, 1639-1699. Malebranche, 1638-1715. Boileau, 1636-1711.	Origin of Whig and Tory, 1680. English Revolution, 1688.
1680	Baglivi, *A.* and $P._2$, 1669-1707. Ruysch, *A.*, 1638-1731. (*Art of injecting blood vessels.*) Peyer, *A.* and $P._2$, *b.* 1653. Lancisi, *A.*, 1654-1720. Brunner, *A.* and $P._1$, *b.* 1653.	Newton, *M.* and $P._3$, 1642-1727. ("Principia," 1687.) Leibnitz, P_3. and *M.*, 1646-1716. James Bernoulli (I.), $P._3$, 1654-1765. Radcliffe, $P._2$, 1650-1714. Mariotte, $P._3$, *d.* 1684.	Locke, 1632-1704. ("Essay," 1690.) Massillon, 1663-1742. Purcell, 1658-1695.	Peter the Great, 1689-1725. Rebellion of Monmouth, 1685. First Russo-Turkish War, 1687. First attempt to light streets of London, 1684.
1690	Valsalva, *A.*, 1666-1723. Clopton Havers, *A.* and *S.*, about 1691. J. M. Hoffmann, *A.*, 1653-1727. Verheyen, *A.*, about 1693. Vallisnieri, *A.* and *B.*, 1661-1730 G. Bartholin, *A.*, 1655-1738. Parfour du Petit, *A.* and $P._1$, 1674-1750.	Stahl, $P._2$, 1660-1734. Halley, *As.*, 1656-1742. De Moivre, *M.*, 1667-1754. F. Hoffmann, $P._3$, 1660-1742.	Sir C. Wren, 1632-1723. (*St. Paul's completed, 1708.*) Bentley, 1662-1742.	William III., 1689-1702. First Fire Insurance, 1696. National Debt begins, 1693. Darien Scheme, 1698. Bank of England, 1694.

1700	Boerhaave, $P._1$ and $P._2$, 1668-1738. Keill, $P._1$ and $P._2$, 1673-1719.	D'Ons en Bray, $P._3$, 1678-1753. Stephen Gray, $P._3$, *d.* 1736. Mead, $P._2$, 1673-1754. Manfredi, $P._2$, 1674-1739.	Defoe, 1661-1731. ("Robinson Crusoe," 1719.) Swift, 1667-1745. ("Gulliver's Travels," 1726.) Addison, 1672-1719. ("Spectator," 1711.) Steele, 1671-1729. ("Tatler," 1709.)	Union with Scotland, 1707. Discovery of Herculaneum, 1708. Newcomen's steam engine, 1705.
1710	Stephen Hales, *N.* and $P._3$, 1677-1761. (*Hydraulics of the circulation.*) Morgagni, *A.*, 1682-1771. (*Beginning of pathology.*)	Jurin, $P._3$, 1684-1750. Papin, $P._3$ and *C.*, 1647-1710. Cotes, $P._3$, 1682-1716. Nicholas Bernoulli (I.), M. and $P._3$, 1687-1759. Cheselden, *S.*, 1688-1752.	Pope, 1688-1744. Le Sage's "Gil Blas," 1715. Watteau, 1684-1721.	George I. Walpole, 1721-1742. Law's Missisippi Scheme, 1717-1720. Treaty of Utrecht, 1713. General Post Office, 1710.
1720	Réaumur, $P._1$ and *C.*, 1683-1757. Pacchioni, *A.*, *b.* 1695.	Maclaurin, *M.*, 1698-1746. Bradley, *M.*, 1692-1762. John Bernoulli (I.), *M.* and $P._3$, 1667-1748.	Berkeley, 1684-1753. J. S. Bach, 1685-1750. Handel, 1685-1759. Peroglesi, 1707-1736. Montesquieu, 1689-1755.	Louis XV., 1723-1774. Foundation of Guy's Hospital, 1724. George II., 1727-1760.
1730	Albinus, *A.*, 1697-1770.	Linnæus, *B.* and *N.*, 1707-1778. Maskeleyne, $P._3$, 1732-1811. Hawksbee, $P._3$ and *As.*, about 1731. Dollond, $P._3$, 1706-1761. Euler, *M.*, 1707-1783. Daniel Bernoulli (I.), $P._3$ and *M.*, 1700-1752. Cramer, *M.* and $P._3$, 1704-1752.	Wesley, 1703-1791. Voltaire, 1694-1778. Metastasio, 1698-1782. Hartley, 1705-1757. Bishop Butler, 1692-1752. Richardson, 1689-1761.	Porteous Mob, 1736. Canton's first electrometer, about 1732. George Washington, 1732-1799. "Gentleman's Magazine," 1731. Coal used to smelt iron, 1740.

Period.	Anatomists and Physiologists.	Representatives of Collateral Sciences.	Representatives of Philosophy, Literature, and Art.	Collateral Events.
1740	Haller, *A.*, $P._1$, and $P._2$, 1708-1777. (*Theory of muscular irritability.*) Whytt, $P._1$ and $P._2$, 1714-1766 Gaubius, $P._1$ and $P._2$, 1705-1780 Needham, *N.*, 1713-1781. Buffon, *N.*, 1707-1788. Trembley, *N.*, 1700-1784. Lieberkühn, *A.*, 1711-1756. J. F. Meckel, *A.*, 1713-1781.	D'Alembert, *M.*, 1717-1783. Clairault, *M.*, 1713-1765. Simson, *M.*, 1687-1768. Boscovitch, *M.*, 1711-1787. Kästner, $P._3$, 1719-1800. Lacaille, $P._3$, 1713-1762. John Bernoulli (II.), *M.* and $P._3$, 1740-1790. Fothergill, $P._2$, 1712-1780.	Fielding, 1707-1754. Hogarth, 1697-1764. David Garrick, 1716-1779. Condillac, 1715-1780. Rousseau, 1712-1778. ("Contrat Social," 1762.) Swedenborg, 1689-1772.	Peace of Aix-la-Chapelle, 1748. The "'45" Rebellion in Scotland. Clive in India, 1750-1760.
1750	William Cullen, $P._1$ and $P._2$, 1712-1790. Bonnet, *N.*, 1720-1793. Zinn, *A.*, 1727-1759. Camper, *A.*, 1722-1789.	Smeaton, *E.*, 1714-1792. Baumé, $P._3$ and *C.*, 1728-1804.	Hume, 1711-1776. Johnson, 1709-1784. (Dictionary begun, 1755.) Diderot, 1713-1784. (Vol. I. "Encyclopédie," 1751.) Smollet, 1721-1771. Lessing, 1729-1781. Gainsborough, 1727-1788. Reynolds, 1723-1792.	Discovery of Pompeii, 1750. Seven Years' War, 1756-1763. First Pitt, 1708-1778. Battle of Plassey, 1757. Wolfe's death at Quebec, 1759. Kaye's "fly-shuttle" in cotton spinning, 1750. Brit. Museum opened, 1759.
1760	John Hunter, *A.*, $P._1$ and *S.*, 1728-1793. (*Action of blood vessels.*) Spallanzani, *N.* and $P._1$, 1729-1799. (*Discoveries in digestion, respiration, generation.*)	Mesmer, *N.* and $P._1$, 1733-1815. Borda, $P._3$, 1733-1799. Lalande, *M.*, 1732-1807. James Watt, *E.*, 1736-1819. Hutton, $P._3$, 1726-1797. Lavoisier, *C.*, 1743-1794.	Thomas Reid, 1710-1796. Adam Smith, 1723-1790. ("Wealth of Nations," 1776.) Gilbert White, 1720-1793. Gibbon, 1737-1794. Lavater, 1741-1801. Chatterton, 1752-1770.	Birth of Napoleon and Wellington in 1769. George III., 1760-1820. Hargreave's "spinning jenny," 1767. Arkwright's "jenny," 1769.

	Galvani, $P._1$ and $P._3$, 1737-1798. (*Animal electricity.*) Hewson, $P._1$ and $P._2$, 1739-1774. (*Functions of blood glands.*) Unzer, $P._2$, 1727-1799. Erasmus Darwin, *N.*, $P._1$, and $P._2$, 1731-1802.	Joseph Black, *C.* and $P._2$, 1722-1799. (*Discovery of carbonic acid, 1754.*) Wedgwood, 1730-1795. Coulomb, $P._3$, 1736-1806. Lagrange, *M.*, 1736-1813. Sir W. Herschell, *As.*, 1738-1822 Bailey, *M.*, 1736-1793. Franklin, $P._3$, 1706-1790. William Hunter, $P._1$ and $P._2$, 1718-1783. Robison, $P._3$, 1739-1805. Biot, $P._3$, about 1774. De Saussure, $P._3$, 1740-1799.	James Boswell, 1740-1795.	Watt's separate condenser, 1769. Watt's double-acting engine, 1782. Bridgewater Canal, 1761. Stamp Act, led to Amer. War, 1765.
1770	Vic d'Azyr, *A.*, 1748-1794. Scarpa, *A.*, 1747-1832. Volta, $P._3$, 1745-1826. Araldi, $P._1$, 1740-1843. Lamarck, *N.* and *Ph.*, 1744-1829. (*Theory of development.*)	Cavendish, *C.*, 1731-1810. Sir J. Banks, *B.*, 1743-1820. Gmelin, *C.*, 1748-1840. John Bernoulli (III.), *M.*, 1744-1807. Atwood, $P._3$, 1745-1807. Priestley, 1733-1804. (*Oxygen discovered, 1774.*) Playfair, $P._3$, 1748-1810. Venturi, $P._3$, 1746-1822. Berthollet, *C.* and $P._3$, 1748-1822 Scheele, *C.*, 1742-1786. Bramah, $P._3$, 1749-1814. Daniel Bernoulli (II.), $P._3$, 1751-1834. Fourcroy, *C.*, 1755-1809. J. Brown, $P._2$. (*Brunonian School.*)	Goldsmith, 1728-1774. Cowper, 1731-1800. Burns, 1759-1796. Kant, 1724-1804. Gluck, 1714-1787. Mozart, 1756-1791. Jacobi, 1743-1819. Sir W. Jones, 1746-1794. (*Introd. Sanskrit to Europe.*) Hannah More, 1745-1833.	Crompton's "mule-jenny," 1775. American War, 1775-1783. Declar. of Independence, 4th July, 1776. Asiatic Society established at Calcutta, 1784.

Period.	Anatomists and Physiologists.	Representatives of Collateral Sciences.	Representatives of Philosophy, Literature, and Art.	Collateral Events.
1780	Prochaska, $P._1$ and $P._2$, 1749-1820. Sömmering, *A.*, 1755-1830. Mascagni, *A.*, *b.* about 1752. Blumenbach, *N.*, 1752-1840. Gall, $P._1$ and $P._2$, 1758-1828. (*Phrenology.*)	Argand, *C.*, 1755-1803. Babbington, *B.*, 1757-1833. Hayley, *As.*, 1749-1820. Laplace, *M.* and $P._3$, 1749-1827. Legendre, *M.*, 1752-1833. Count Rumford, $P._3$, 1753-1814 Pierre Prevost, $P._3$, 1751-1839. J. P. Prevost, *C.*, 1755-1819. Olbers, $P._3$, 1758-1840. Carnot, *M.* and $P._3$, 1753-1823. James Bernoulli (II.), $P._3$, 1759-1789. Telford, *E.*, 1757-1834. Chladni, $P._3$, 1756-1827. Daguerre, *C.*, 1789-1851.	Sheridan, 1751-1816. Dugald Stewart, 1753-1828. Paley, 1743-1805. Wieland, 1733-1813. Vernet, 1758-1835. David, 1748-1825. Haydn, 1732-1809.	America recognised, 1783. Second Pitt, 1759-1806. Fox, 1749-1806. Edmund Burke, 1729-1797. Linnæan Society founded, 1788. Cartwright's steam power-loom, 1784. Cort's puddling process, 1784. Discovery that electricity decomposed water, by Päts van Trostwyk, 1789. States-General meeting, 1789.
1790	Bichat, $P._1$, 1771-1802. (*Life of tissues.*) Girtanner, $P._1$ and $P._2$, about 1790. Kielmeyer, $P._2$, about 1790. Aldini, $P._3$, 1762-1834. (*Gas obtained from blood* by Sir H. Davy in 1799.)	Fourier, *M.*, 1772-1837. Brunel, *E.*, 1769-1849. Leslie, $P._3$, 1766-1832. W. Humboldt, $P._3$, 1767-1835. A. von Humboldt, $P._3$ and *N.*, 1769-1859. Haüy, $P._3$, 1743-1822. Ivory, *M.*, 1765-1842. Playfair, $P._3$, 1749-1819. Dalton, *C.*, 1766-1844. Jussieu, *B.*, 1748-1836. Cuvier, *C.A.*, 1769-1832. Ampère, $P._3$, 1775.1836. Jenner, $P._2$, 1749-1823. (*Vaccination, 1796.*)	Jeremy Bentham, 1748-1832. Goethe, 1749-1832. Jean Paul Richter, 1763-1825. Walter Scott, 1771-1832. Schiller, 1759-1805. Mrs. Siddons, 1735-1831. Flaxman, 1755-1826. Canova, 1757-1822. Beethoven, 1770-1827. Weber, 1786-1826. William Blake, 1757-1827. Schleiermacher, 1768-1834. Malthus, 1766-1834. Fichte, 1762-1814.	Republic proclaimed, 1792. The French Revolution, 1790. Louis XVI. guillotined, 1793. Robespierre ,, 1794. Washington, 1732-1799. Napoleon, 1769-1821. Battle of the Nile, 1798. Coal-gas used for lighting, 1792. Howard died at Kherson, 1790.

1800	Thomas Young, $P._1$, $P._2$, and $P._3$, 1773-1829. (*Measurement of time, theory of colour, &c., hydraulics of circulation.*) J. F. Berard, $P._1$, 1780-1828. Rudolphi, *A.*, 1771-1832. Charles Bell, $P._1$ and *S.*, 1774-1842. (*Sensory and motor nerves.*) Richerand, $P._1$ and $P._2$, 1779-1840. Bostock, $P._1$, 1774-1846. Treviranus, $P._1$, 1776-1837. Spurzheim, $P._1$ and $P._2$, 1776-1832. (*Phrenology.*)	Kater, $P._3$, 1777-1835. Barlow, $P._3$, 1776-1862. Gauss, $P._3$, 1777-1855. Pfaff, *C.* and $P._3$, 1773-1852. Baily, $P._3$, 1774-1844. Malus, $P._3$, 1775-1812. Seebeck, $P._3$, 1770-1831. Oersted, $P._3$, 1777-1851. Arago, *As.*, 1786-1853. Thomas Thomson, *C.*, 1773-1852. Robert Brown, *B.*, 1773-1858.	Coleridge, 1772-1834. Wordsworth, 1770-1850. Byron, 1788-1824. Madame de Stäel, 1766-1817. Hegel, 1770-1831. Grimm, 1785-1863. Lamb, 1775-1834. Schlegel, Fried. v., 1772-1829. (*Founded Science of Language.*)	Union of Great Britain and Ireland, 1801. "Edinburgh Review," 1802. Assaye, 1803. Trafalgar, 1805. Nelson, 1758-1805. French Empire, 1804-1815. Austerlitz, 1805. Jena, 1806. Peninsular War, 1808-1814. Wellington, 1769-1852. Trevithick's first steam locomotive, 1804. First steamboat on Forth and Clyde Canal, 1802. Electrotyping invented, 1805.
1810	Edwards, *N.* and $P._1$, 1777-1842. Purkinje, $P._1$, *b.* about 1787. Sir B. Brodie, $P._1$ and *S.*, 1783-1862. Dutrochet, $P._3$, 1776-1847. Magendie, $P._1$, 1783-1855. (*Theories of absorption.*) G. Breschet, *P.*, 1784-1845. Meckel, *A.*, 1781-1833. E. Home, *C.*, *A.*, and *S.*, 1756-1832.	Peltier, $P._3$, 1785-1845. Döbereiner, *C.* and $P._3$, 1780-1849. Hare, $P._3$, 1781-1858. C. Ritter, $P._3$, 1779-1859. Gay Lussac, *C.*, 1778-1850. G. Stephenson, *E.*, 1781-1848. (*Locomotive.*) Fresnel, $P._3$, 1783-1827. Niepce, $P._3$, 1765-1833. Sir Humphry Davy, *C.*, 1778-1829. (*Safety lamp, 1815.*)	Shelley, 1792-1822. Keats, 1795-1821. Schelling, 1775-1854. Niebuhr, 1776-1831. Schopenhauer, 1788-1860. Ricardo, 1772-1823. Beranger, 1780-1857. Thorwaldsen, 1770-1844. Wilkie, 1785-1841. Turner, 1775-1851.	Invasion of Russia, 1812. Abdication of Napoleon at Fontainbleau, 1814. Battle of Waterloo, 1815. Talleyrand, 1754-1838. First electric arc light, 1810. Lithography, 1811.

Period.	Anatomists and Physiologists.	Representatives of Collateral Sciences.	Representatives of Philosophy, Literature, and Art.	Collateral Events.
1810	L. Oken, *A.*, 1779-1848.	Wollaston, $P._3$, 1766-1828. Frauenhofer, $P._3$, 1787-1826. Bessel, *M.* and $P._3$, 1784-1846. Nobili, $P._3$, 1784-1835. Ohm, $P._3$, 1787-1854. Christopher Bernoulli, $P._3$, *b.* 1782. Braconnet, $P._3$, 1781-1855. Brande, *C.*, 1788-1866. Cagniard de la Tour, $P._3$, *b.* 1776 Chevreul, *C.*, 1786-1888. Buckland, *N.*, 1784-1856. A. C. Becquerel, *C.* and $P._3$, 1788-1878. Berzelius, *C.*, 1779-1848. De Candolle, *B.*, 1778-1841.	Champollion, 1790-1831. (*Deciphered Hieroglyphics, 1822.*)	Bell's "Comet," first paddle steamboat on Clyde, 1812. Oersted's discovery of electro-magnetism, 1819. Sömmering's first telegraph by electrolysis, about 1809. Queen Victoria born 1819.
1820†	Krause, *A.*, 1797-1868. Beaumont, $P._2$, about 1824. (*Digestion.*) Gmelin, *C.*, 1788-1853. Serres, *A.*, 1782-1862. E. H. Weber, $P._1$, 1795-1878. (*Circulation, muscular action.*) J. L. Prevost, *C.*, 1790-1850. Von Baer, *A.* and $P._1$, 1792-1876. Marshall Hall, $P._1$ and $P._2$, 1790-1857. (*Reflex nervous action.*)	Basevi, $P._3$, *b.* 1799. Despretz, $P._3$, *b.* 1792. Chasles, *M.*, 1793-1880. Struve, $P._3$, 1793-1864. Mary Somerville, *M.* and $P._3$, 1780-1872. Daniell, $P._3$ and *C.*, 1790-1845. Cauchy, *M.*, 1789-1857. Encke, *As.*, 1791-1865. Babbage, $P._3$, 1792-1871. Savart, $P._3$, 1791-1841.	Comte, 1798-1857. Whewell, 1794-1866. Sir W. Hamilton, 1788-1856. Thomas Chalmers, 1786-1847. Macaulay, 1800-1859. Heine, 1800-1856. Guizot, 1787-1874. Balzac, 1779-1850. Etty, 1782-1789. Chantrey, 1781-1841. Ary Scheffer, 1795-1858.	Charles X., 1824. Nicholas I., 1825-1855. Navarino, 1827. Insurrection in Poland, 1830-1831. Canning, 1770-1827. Catholic Emancipation, 1829. George Stephenson's first steam engine on railway, 1829. Hot-blast invented, 1828. Money panic, 1825.

	Flourens, $P._1$, 1794-1867. Ehrenberg, *N.*, 1795-1876. Poiseuille, $P._3$, 1799-1869. (*Use of manometers in investigating circulation.*) Dupuy, $P._1$ and $P._2$, 1774-1849.	Mitscherlich, $P._3$ and *C.*, *b.* 1794. Audouin, *N.*, 1797-1841. Poggendorff, $P._3$, 1796-1877. Payen, *C.*, *b.* 1796. Frauenhofer, 1787-1826. Bischof, *C.*, *b.* 1792. Moebius, *C.* and $P._3$, *b.* 1790.	Delaroche, 1792-1852. Rossini, 1792-1868. Spohr, 1784-1871. Donizetti, 1798-1848. Schubert, 1797-1828. Bellini, 1806-1835. Mendelssohn, 1809-1847. Meyerbeer, 1794-1864.	Caledonian Canal, 1824.
1830	Johann Müller, $P._1$, 1801-1858. Schleiden, *B.*, 1804-1872. (*Cell theory.*) Volkmann, $P._1$, 1801-1877. Schroeder van der Kolk, *A.* and $P._1$ 1797-1862. Matteucci, $P._1$ and $P._3$, 1811-1869. Magnus, *C.* and $P._1$. (*Collection of blood gases in 1836.*)	C. Lyell, *G.*, 1797-1875. Faraday, *C.* and $P._3$, 1794-1867 Sir W. Rowan Hamilton, *M.*, 1805-1865. Colladon, $P._3$, *b.* 1802. Sturm, $P._3$, 1803-1855. Lassaigne, $P._3$, *b.* 1800. Frémy, $P._3$ and *C.*, *b.* 1814. Listing, *M.*, *b.* 1808. Airy, *As.*, *b.* 1801. Melloni, $P._3$, 1798-1853. Amici, $P._3$, *b.* 1786. Sir John Herschell, *As.* and $P._3$, 1792-1871. Balard, *C.*, *b.* 1802. Von Bibra, $P._3$ and *C.*, *b.* 1806. Boussingault, *C.*, *b.* 1802. Challis, *M.*, 1803-1882. Christison, $P._1$ and $P._2$, 1779-1880. Mulder, *C.*, *b.* 1802. Gassiot, $P._3$, 1797-1877. Dumas, *C.*, 1806-1884. Sir D. Brewster, $P._3$, 1781-1868	Thomas Arnold, 1795-1842. J. S. Mill, 1806-1873. Chopin, 1809-1849. Milman, 1791-1868. Kèble, 1792-1866. Landseer, 1802-1873. Schumann, 1810-1856.	Liverpool and Manchester Railway, 1830. Abolition of Slavery, 1834. William IV., 1830-7. The Reform Bill, 1830-1832. Queen Victoria's reign begins, 1837. Faraday's discovery of induction, 1831. Salvatore da Negro's first electro-motor, 1830. Jacobi's electro-motor applied to propel vessel on Neva, 1834. Reis' first telephone, 1837. Gauss and Weber's electro-magnetic telegraph, 1833. Improvements in telegraphy by Wheatstone and Morse, 1837 to 1839. "Chambers' Journal," 1832.

Period.	Anatomists and Physiologists.	Representatives of Collateral Sciences.	Representatives of Philosophy, Literature, and Art.	Collateral Events.
1840†	Claude Bernard, $P._1$, 1813-1878. (*Vaso-motor nerves, &c., &c.*) Schwann, *A.* & $P._1$, 1810-1882. (*Cell theory.*) John Reid, $P._1$, 1809-1849. John Goodsir, *A.* and $P._1$, 1814-1867. (*Secretion, &c., &c.*) Fechner, *M.*, $P._3$, and *Ph.*, 1801-1887. (*Psycho-physik.*) Hutchison, $P._2$, about 1846. Allen Thomson, *A.* and $P._1$, 1809-1884. Carpenter, $P._1$, 1813-1885. Bowman, $P._1$ and *S.*, 1816. Owen, *C.A.*, 1804. Charles Darwin, *N.* and *Ph.*, 1809-1882. Henle, *A.*, *b.* 1809. Ranke, *A.* Sharpey, $P._1$, 1802-1880. Hughes Bennett, *P.* and $P._2$, 1812-1875. Andrew Buchanan, $P._1$, 1798-1882. (*Coagulation of Blood.*)	Foucault, $P._3$, 1819-1868. Gorup Von Besanez, *C.*, 1817-1878 Thomas Graham, *C.* and $P._3$, 1805-1869. Lord-Justice Grove, $P._3$, 1811. A. W. Hofmann, *C.*, 1818. J. P. Joule, $P._3$, 1818-1889. Lehmann, *C.*, 1812-1863. W. H. Miller, *C.*, 1801-1880. De Morgan, *M.*, 1806-1871. Plateau, $P._1$, 1801-1884. J. R. Mayer, $P._3$ & $P._2$, 1814-1878 Regnault, *C.*, 1810-1878. Leverrier, *As.*, 1811-1877. Adams, *As.*, *b.* 1819. Liebig, *C.*, 1803-1873. Draper, $P._3$ and $P._2$, 1811-1882. Wheatstone, $P._3$, 1802-1875. Edward Forbes. *N.*, 1815-1854. Agassiz, *N.*, 1808-1873. Andrews, $P._3$ and *C.*, 1813. Angström, $P._3$, 1814-1874. Bunsen, *C.*, 1811. Cahours, $P._3$, *b.* 1813. Dana, *N.*, *b.* 1813. Kopp, *C.*, *b.* 1817. Wertheim, $P._3$, 1815-1861. Von Mohl, *B.*, 1805-1872. Simpson, $P._3$, 1811-1870. (*Chloroform.*)	T. Carlyle, 1795-1884. Wagner, 1813-1883. Liszt, 1811-1887. J. F. Millet, 1814-1875. Victor Hugo, 1802-1886. G. Sand, 1804-1870. J. S. Mill, 1806-1875. E. B. Browning, 1809-1861.	War in Scinde, 1843. Free Church Disruption, 1843. Last voyage of Sir J. Franklin, 1845. Repeal of Corn Laws, 1846. Nasmyth's steam hammer, 1842. Foucault's regulator for arc light, 1844. Steinheil's (1801-1870) first writing telegraph, about 1848. First printing telegraphs, Bail (1837), Bain, 1840, and Wheatstone, 1841. First cable telegraphs, Morse, 1842, Cornell, 1845. Free Trade, 1849. Year of Revolution, 1848.

1850	Ludwig, $P._1$, *b.* 1816. Helmholtz, *M.*, $P._3$, and $P._1$, 1821. Donders, $P._1$ and *S.*, 1818-1890. Brücke, $P._1$, 1819. Brown-Sequard, $P._1$ and $P._2$, *b.* 1818. Du Bois-Reymond, $P._1$, 1818. Schiff, $P._1$, *b.* 1823. Joseph Lister, $P._1$ and *S.*, 1827. *(Antiseptic surgery.)* Vulpian, $P._1$, 1826-1887. Vierordt, $P._1$, 1818-1884. Huxley, *N.*, *A.*, and $P._1$, 1825. Pettenkofer, *C.* and $P._1$, *b.* 1818. Lothar Meyer, *C.* and $P._1$, *b.* 1830. Turner, *A.* Ogilvie-Forbes, $P._1$, 1820-1886 Struthers, *A.*	Stokes, *M.* and $P._3$, 1819. W. Thomson, *M.* and $P._3$, 1824. Hind, *As.*, *b.* 1823. A. E. Becquerel, $P._3$, 1878. Berthelot, *C.* and $P._3$, *b.* 1827. Cayley, *M.*, 1821. Sylvester, *M.*, 1814. Clausius, *M.* Tyndall, $P._3$, *b.* 1820. Kekule, *C.*, 1829. Kirchoff, $P._3$, 1824-1887. *(Spectroscope.)* Syme, *S.*, 1800-1870. Macquorn Rankine, *E.*, 1820-1872. Haeckel, 1834. Knoblauch, $P._3$. Moleschott, *C.*, *b.* 1822. Pasteur, *C.* and $P._1$, 1822. *(Bacteriology.)* Virchow, 1821. *(Pathologist.)* Clerk-Maxwell, *M.* and $P._3$, 1831-1879.	Thackeray, 1811-1863. Dickens, 1812-1870. Marian Evans, 1819-1880. (George Eliot.) D. G Rossetti, 1828-1882. Rubinstein, 1829. Sainte-Beuve, 1804-1869. Von Ranke, 1795-1886. John Brown, 1810-1882. "Horæ Subsecivæ." Buckle, 1828-1882. Cardinal Newman, 1801-1890. Tennyson, 1809. R. Browning, 1812-1890. Longfellow, 1807-1882. Oliver Wendell Holmes, *b.* 1809. Walt Whitman, *b.* 1819.	Crimean War, 1853-1856. Indian Mutiny, 1857-8. Franco-Italian-Austrian War 1859. Bessemer steel process, 1856. Dynamo-electric machines, about 1858. First submarine telegraph, from Dover to Calais, 1857. Great Exhibition, 1851. Pacinotti's invention of ring-armature in dynamos, 1860. First steps towards an Atlantic cable by Cyrus Field, on 1854. First attempt to lay Atlantic cable, 1857, second in 1858. First telegram across Atlantic, 7th August, 1858.
1860				

† The dates of comparatively recent authorities have not been in all cases ascertained.

VI.—*The Oyster Fishery of Scotland.* By J. H. FULLARTON, M.A., D.Sc., Zoologist to the Fishery Board for Scotland.

[Read before the Society, 7th January, 1891.]

THE question of the oyster supply has been much oftener brought before the public in England and Ireland than in Scotland, but there are now signs that the interest of Scotchmen in this subject is reviving, and that the listlessness and apathy of our countrymen are being banished by the information, which is now and again given by the public press, as to the methods which have been successfully employed in America, France, and Holland for increasing the supply of oysters. It is, therefore, not inappropriate that the members of the Philosophical Society of Glasgow should consider a question involving a possible increase of the national revenue, which, at one and the same time, would directly benefit both the fishing communities of Scotland and the consumers of this delicious bivalve. A number of Commissions have investigated the subject of our oyster fisheries, but their attention has chiefly been directed either to the English or the Irish oyster fisheries, and next to nothing was known to them, or, if so, published, about the Scottish oyster fishery. It has recently been my good fortune to visit the chief inlets on the West of Scotland where oysters could formerly be obtained, if not in great abundance, at least in large numbers; and, though I do not propose to lay before you the detailed results of my examination into the present condition of our various western lochs, it is open to me to compare the actual condition of our oyster fisheries with what, so far as the records permit, we can ascertain the past to have been, and to state some obvious lessons.

The Select Committee of the House of Commons appointed in 1876 to report on the Oyster Fisheries, came "to the conclusion that the supply of oysters round the British coasts has for some years steadily decreased." The statistics of the Scottish fisheries in recent years show that this is true not only for oysters but also for most other classes of shellfish. A memorandum on the

present state of the shore and sea fisheries of Scotland, drawn up a month ago by Dr. T. Wemyss Fulton, of the Fishery Board for Scotland, after furnishing a commentary on the statistics, concludes "that the shore fisheries, especially those for lobsters, crabs, oysters, and mussels are steadily declining." Now, it is in connection with the shore fisheries that the agency of man is chiefly felt. Man can appreciably diminish the supply of lobsters, crabs, oysters, and mussels which are taken, for the most part, from inshore waters, and conversely man can assist nature in increasing the available supply of them. Some years ago the assumption that man, by anything he can do, could diminish the fish supply, was regarded as a heresy; but the Scottish lobster, oyster, and mussel fisheries afford a proof that it is possible, by overfishing and by reckless neglect of biological laws, to impoverish and destroy most valuable fishings, and bring ruin to many whose means of subsistence largely depend on a plentiful supply from them. Dr. Fulton, in his memorandum, shows that in Scotland out of every £25 worth of produce taken from the sea, only £1 worth of that represents the proportion taken from the shore fisheries, while in France, out of £4 10s. yielded by the shore and sea fisheries, £1 of that represents the value of the shore fisheries. The annual value of the French shore fisheries is about £850,000, while that of Scotland is less than one-twelfth of that amount. Unfortunately, our Scottish statistics do not go back beyond 1883, but contrasting the first three years with the past three years, we find that the annual average value has decreased from £84,359 to £67,414, so that the decline in value of our shore fisheries in these seven years has been at least 20 per cent.

If we view the oyster question apart from the other shore fisheries we find the decline even more marked. The statistics of oysters obtainable from the annual reports of the Fishery Board give the yield in quantities and values as follows :—

Years.		Quantities.		Values.
1883	...	645,600	...	£3,406
1884	...	517,600	...	2,174
1885	...	220,200	...	809
1886	...	295,650	...	1,295
1887	..	213,000	...	665
1888	...	152,700	...	742
1889	...	311,933	...	1,453

The first three years show an average yield of 461,100, of the value of £2,129; and the last three give an annual average of 225,800,

of the value of £1,053, or a decrease of more than 50 per cent. both in quantity and value. The *Statistiques des pêches maritimes* give the produce of French oyster fishery for 1887 (the last year available) as 759,930,600, of the value of £500,000. Before inquiring into the reasons for this disproportion between the Scottish and French yields of oysters, we may seek to gather the information which is available in regard to the past of our oyster supply. This takes us at once to the most reliable source of information regarding the whole condition of the Scottish people—namely, the "Statistical Account of Scotland" by Sir John Sinclair, published at the end of last century. Although the facts available are not so numerous as one might desiderate, especially in reference to our western seaboard, yet such as are given show that the sources of our oyster supply were in a much better condition than they are now.

Every one acquainted with the "Statistical Accounts" knows that the statements given by the parish ministers are very unequal; some clergymen revel in detail, while others are content with the baldest outline. The condition of the oyster fishery is best narrated by the clergymen of the districts in the neighbourhood of the Forth. One could have wished that the Highland ministers had been equally elaborate in their account of the fishing in the western lochs. As it is, in many cases they simply mention the fact of oysters being found without saying anything more about them.

Taking the best descriptions first, the following is the account given of Cramond*:—"The fisheries are at a low ebb, the oyster fishery being much degenerated from what it was about 50 years ago, when 11 large boats belonging to Cramond were constantly occupied during the season in dragging oysters, the greatest part of which was sold to Dutch vessels at an average of 4s. the herring barrel. The scalps were then so productive that it was usual for a boat with five hands to make 30s. a day. But they are now so much destroyed, probably with overfishing, as to employ at present no more than four or five boats, and these only occasionally. The scalps about Inch Mickry, belonging to Lady Greenwich, are let at a rent of £24 per annum to the Newhaven fishers."

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* The "Statistical Account of Scotland." By Sir John Sinclair. Edin. Vol. i., p. 220.

The minister of the Parish of Tranent says* :—"Off the coast (Cockenzie) there is a considerable oyster scalp, but of late years the oysters have become very scarce. This must be attributed to overdragging several years ago to supply English vessels from Milton, Lee, and other places. Before this a boat could sometimes drag 9,000 a day, which at 5d., 6d., and 7d. afforded a handsome income to the crew. The common average lay betwixt 4,000 and 7,000. At present 700 or 800 are reckoned a good day's work, which, though now sold indiscriminately at 15d. per hundred, afford but a scanty livelihood when divided amongst five men, including a double deal to the owner of the boat. There are at present ten boats belonging to Cockenzie and Port Seton engaged in this fishing. Upwards of 20,000 oysters go regularly each season to the Glasgow market. (The demand from Glasgow has not been so great this year, but some of the boats have found a ready market and good price at Newcastle, to which they have performed several trips during the harvest months.)"

The fullest information in regard to the celebrated Forth Pandore oysters is vouchsafed by the minister of Prestonpans. He states† :—"The chief fishing is that of oysters. There are at present ten oyster boats belonging to the parish. Each boat requires five men, but the profits are divided into six shares, one share being applied for upholding the boat. There are not, however, above 23 regular fishermen; all the others work occasionally on land or sea as they find for their advantage. A boat seldom returns with more than 400 or 500. The present price is 15d. per hundred. . . . Three or four times in a season a boat sails with a cargo of them, to the number of 30,000, sometimes 40,000, to Newcastle. . . . The present price at Newcastle is 2s. per hundred. Oysters are carried to Glasgow by land. Two carriers with four one-horse carts come from Glasgow to Edinburgh with goods, and return loaded with oysters, which they purchase at Prestonpans and Cockenzie. The medium is about 9,000 in each cart. . . . Some of the aged inhabitants report that from 60 to 70 years ago oysters were in little estimation. In a judicial declaration emitted A.D. 1776, by a residenter here, then 67 years old, he deponed 'that he remembered when there were not above three or four boats employed; that they seldom caught above 600 in a day; and that there was little

* *Loc. cit.*, vol. x., p. 86.

† *Loc. cit.*, vol. xvii., p. 69.

or no demand or sale for them at that period. . . . About twenty years ago the scalps were so productive that 6,000 oysters and upwards were frequently dragged by one boat in a day. The price at that time was 6d. per hundred. Besides the consumption in the neighbourhood, they were exported to Newcastle, Hull, and London. A merchant at Leith, in the year 1773, contracted to ship oysters on commission for London. He purchased for ten different companies, and for ten years paid £2,500 sterling per annum for oysters. The value of the home consumption was estimated to be still greater. Forty boats were then employed. . . . The oysters for the London market were packed in barrels. Twelve vessels were employed in the trade from the middle of January to the middle of May. Each vessel carried at a medium 320 barrels; each barrel was supposed to contain 1,200 sizeable oysters. . . . Thirty cargoes have been shipped in a season. The oysters were dropped in bays at the mouth of the Thames and Medway, and other grounds, to fatten until the fall, when they were dredged and sent to market. This trade was given up in the year 1786 owing to the scarcity and advanced price of oysters, the price having risen from 4s. 6d. to 7s. and 8s. per barrel. . . . The scalps are greatly exhausted by this trade."

The Burntisland chronicler states*:—"Excellent oysters are also to be had near the town. The bed belongs partly to the burgh, and partly to the Earl of Morton."

These descriptions of the Forth oyster beds are in striking contrast to the position to-day. Last year the total value of the oysters obtained in the Forth amounted to £175. One hundred and twenty years ago the quantity dredged from a portion of this ground—that on the south side of the Firth below Inchkeith—was estimated at 22,000,000 oysters, which, at the very moderate figure of 10s. per hundred, would represent to-day a value of £110,000. If to these figures were added the quantities obtained by the fishermen further up the Firth and on the north side, the yield about 1770-1780 would be considerably greater, so that we may safely say for every single oyster now dredged in the Forth 600 to 1,000 were then obtained. I have often worked on these old beds with a large oyster dredge, and half-a-dozen oysters was the greatest number I could obtain after several hours' work.

**Loc. cit.*, vol. ii., p. 428.

It may naturally be asked, what is the reason for the disappearance of the oyster from the Forth. The old "Statistical Account" suggests that the diminished production was due to the non-observance of the close time from the 1st of May to the 1st of September, to the non-compliance with the injunction that none were to be taken under the pattern size furnished to each boat—which seems to have been about an inch and a half in diameter;—and it further suggests*, when the daily catch of each boat had decreased from 9,000 to 400 or 500, "that the scalps may recover, it would be proper to dredge very sparingly for a year or two, to take no oysters but such as are sizeable, and at no time to dredge in the months of May, June, July, and August."

The initial cause of the diminished yield of the beds was undoubtedly due to these reasons, but another cause has operated, and continues to operate, against the recuperation of the oyster beds. What was formerly a great oyster bed is now an extensive bed of clams or scallops (*Pecten opercularis*). An area of over twenty square miles of the bed of the Firth is covered by the clam, and the oyster has been pushed aside by its less delicate but more vigorous fellow-bivalve. The clam has gone on increasing and spreading itself over a wider area, and the food supply for the oyster has in proportion decreased. This is not an unmixed evil, as thereby the fishermen along the neighbouring coast are plentifully supplied with bait for their lines, and during this winter a new industry has developed. The supply of bait from this source used to go entirely to Forth fishermen, but now as many as thirty Newhaven boats are employed dredging for clams, which are forwarded to fishing villages along the east and north-east coasts of Scotland, where the scarcity of the bait supply is being most keenly felt. While, therefore, the oyster produce of the Firth of Forth has decreased, the supply of clams has so increased that the Forth fishermen can supply their own needs and do a little to mitigate the hardship of their more distant fellow-fishermen. It may not, however, be out of place to remind the fisherman that it is quite possible to diminish the clam-supply by overdredging. The clam fishery, no less than the oyster fishery, can be destroyed, and the want of intelligent appreciation of the situation, and of wise regulations, is more urgently required than ever.

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* *Loc. cit.*, vol. x., p. 86.

Proceeding northwards from the Forth, the next place noticed by the old "Statistical Account" where oysters were to be found was the Parish of Craig in Forfarshire, where an attempt was made to introduce the oyster to the basin of Montrose. It is said*:—"A few years ago a quantity of oysters were brought from the Firth of Forth, and put down in a place where they were likely to breed. But there is some reason to suspect, though the point is not yet ascertained, that oysters cannot thrive in the neighbourhood of muscles." The chronicler's suspicion is perfectly correct, as oysters do not thrive alongside of the more vigorous mussels, but are rapidly killed by the mussels weaving their byssus threads over them, thus preventing them getting a supply of food or oxygenated water. But if oysters have not become acclimatised at Montrose, the successful culture of mussels, inaugurated by Mr. James Johnstone upwards of thirty-five years ago, and conducted on an extended scale to-day, removes any regret there may be at the want of success in oyster-breeding in the Montrose basin.

In Orkney and in Shetland there were oyster scalps a century ago, but these have, like the Forth scalps, been depleted and destroyed. In the Parish of Firth and Stenness, Orkney, the minister describes the oyster fishery thus †:—"In this bay (Firth) excellent oisters, and of a large size, are found in tolerable plenty; they are sold at one shilling the hundred."

Not so many years ago oysters "in tolerable plenty" were to be had in Firth Bay, but the Orcadian oyster fishery is now practically, if not altogether, a thing of the past. Taking Shetland, it is said, ‡ in reference to Dunrossness, that "the rocks on the coast produce abundance of lobsters, crabs, oysters, &c.

In Bressay, Barra, and Quarff the "Statistical Account" says §:—"They have on their coast a fine oyster scalp from which they take large rich oysters. Hence they are in general in easy circumstances."

Where one reads such an account as this, and remembers how the condition of the French peasant is helped by the produce of his oyster parcs, one wishes that it could be said of all the places in Scotland where oysters might be fattened, that the people "are in general in easy circumstances."

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* *Loc. cit.*, vol. ii., p. 497.
† *Loc. cit.*, vol. xiv., p. 137.
‡ *Loc. cit.*, vol. xiv., p. 394.
§ *Loc. cit.*, vol. x., p. 202.

While oysters have been present in great abundance on the East of Scotland, the localities where these were found were, and only could be, few in number, it is to the western shores that we must look for the evidence of the presence of oysters in the past over an extended area, and for the possible resuscitation of oyster beds at several points in the future. For convenience the localities may be classed in three divisions: (1) Those south of the Mull of Kintyre, (2) those between Oban and the Mull of Kintyre, (3) those north of Oban.

Loch Ryan and Luce Bay are the two places where oysters are to be obtained to the south of the Mull. Sir William Wallace, of Cairn Ryan, still employs three oyster smacks on the scalp in Loch Ryan, but the Bay of Luce has been so fished out that it is said to yield so few oysters to-day that the fishing would not prove remunerative. The Loch Ryan fishery extends from Cairn Ryan to the pier at Stranraer, and oysters are obtained over all this area. At the end of last century it was said*:—"This bank (the Scar) abounds with oysters of a most excellent flavour. They are found, indeed, all round the shores, and might be got in great quantities would people drag for them. At present they are only gathered, at low water in spring tides, for a few months in the spring."

That was written by the incumbent of Stranraer Parish, and the minister of Kirkcolm says †:—"Beyond a small point of land called the Star,‡ there is a fine bank of most excellent oysters, small indeed, but highly flavoured, and of a most delicious taste. It is believed that, if persons skilled in managing dredge nets were to ply in deep water, oysters would be got in greater numbers and of a larger size than by the present mode of taking them off the beach with the hand when the tide is out; and, instead of being stinted to a day or two about the new or full moon, they might dredge for them at all times with success."

In Kirkmaiden Parish § "The shellfish, oysters, and lobsters are (described as) very good of their kind."

In 1891 the oysters cannot be obtained by gathering on the foreshore, but are got by "persons skilled in managing dredge nets." Sir William Wallace observes a close time from the beginning of May till the end of August, and no oysters of a less

* *Loc. cit.*, vol. i., p. 358.
† *Loc. cit.*, vol. ii., p. 47.
‡ I suppose this means the Scar.
§ *Loc cit.*, vol. i., p. 154.

size than 2½ inches in diameter are taken. Formerly fishermen paid a weekly or monthly lordship, or license money, and they were allowed to take the large oysters, but the proprietor says that "the fishermen have become so clever and so unscrupulous in evading this rule" that he was obliged to take the beds into his own hands.* As much as £616 was obtained in one year (1868) from the license money, and the fishery now (1889) yields £537. It is to be regretted that the oyster fishery has not been developed at Loch Ryan to greater proportions, and that the fishery is left to reproduce "for itself as it has done since 1822." If southern methods were adopted, Loch Ryan might be expected to yield a much richer harvest.

Oysters are noticed by the "Statistical Account" as being obtainable at Lochgoilhead† "on the shells and rocks," but it is when we come to West Loch Tarbert, in Kintyre, that we next hear of oysters being sent to the market‡ :—"Among the curiosities of this parish are immense banks of oyster shells in Loch Tarbert, with which the inhabitants manure their lands." But this is not all, as it goes on to say:—"There are some shellfish, particularly oysters, which are sold on the spot at 6d. the hundred, and sent to the markets at Campbeltown and Greenock." West Loch Tarbert, like Loch Ryan, is one of the places where oysters are still to be obtained in quantity, but the present favourable condition is due to the energy of Messrs. Hay & Co., who a few years ago obtained a Fishery Order from the Fishery Board for Scotland. The proprietors have a limited stretch of ground, and they have laid down on this, and have in stock something like one million of oysters. The yield of the fishery last year was £701.

The "Statistical Account" tells of "great quantities of oysters" § in Loch Tarbert, Jura; of "the finest oysters that are anywhere to be found, and in great plenty," in Lochs Craignish and Crinan; ‖ of oysters in Lochs Melfort,¶ Scridain,** and na-Keal,†† and in other places. This state of matters was very different from what exists at the present day, when, to obtain samples of oysters at

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* *Vide* Report of the Select Committee of the House of Commons on Oyster Fisheries, 1876, Appendix No. 8, p. 237.

† *Loc. cit.*, vol. ii., p. 174.

‡ *Loc. cit.*, vol. x., pp. 56 and 60.

§ *Loc. cit.*, vol. xii., p. 322.

‖ *Loc. cit.*, vol. viii., p. 92.

¶ *Loc. cit.*, vol. x., p. 317.

** *Loc. cit.*, vol. xiv., p. 176.

†† *Loc. cit.*, vol. xiv., p. 140.

the localities mentioned, it requires an amount of time altogether disproportionate to the catch.

The condition of the oyster fishery a century ago was very fair; to-day it has almost reached the vanishing point. Then, a hundred oysters could be obtained for 4d. at Cramond; 5d., 6d., or 7d. at Cockenzie; 6d. at West Loch Tarbert; rising to 1s. 3d. at Prestonpans, and 2s. at Newcastle. Now, good native oysters cost 2d. to 3d. each. Then, a crew of five hands could earn 30s. a day; now, it would puzzle them to earn on the same grounds as many pence, and that, too, even though oysters have increased in price 800 per cent.

It is interesting to notice that while overdredging was taking place on our Scottish beds, the oysters obtained were utilised for the stocking of the Dutch beds and the beds in the Thames estuary. When oysters are dredged for laying down elsewhere, the temptation of immediate profit is too great for the fishermen, and the regulation as to minimum size is almost useless, unless enforced under strict supervision and legal penalties. So, whenever there has been a demand for oyster spat or for oysters for fattening, fishermen have always been found, who, in the absence of prohibitive regulations, would remove the young oysters, to the impoverishment and destruction even of the bed. This is no fanciful explanation of the wholesale destruction of the oyster scalps, as it took place not only at the end of last century, but it has occurred within the last quarter of a century. Sir William Wallace, in his communication to the Select Committee on Oyster Fisheries in 1876, alleges * that the growing scarcity of oysters, the great demand for young oysters, and the facilities for their transit, "have all combined to make the fishermen endeavour to steal the brood." Recently I was informed how some years ago lochs in Mull, Skye, and elsewhere in the west, were dredged and redredged, and every vestige of oyster, except the empty shells, carried away. So much and so successfully has this been done, that many hours' work over the ground of formerly productive lochs reveals of the oyster only what is in the process of becoming converted into fossil remains.

The questions which one naturally asks are, *Is this diminution to go further? is it possible to do anything to combat its effects and increase the supply of oysters? and, if so, how is it to be accomplished?*

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* *Loc. cit.*

Such questions as these were faced by the French authorities from thirty to forty years ago, and subsequently by the Dutch and American Governments. It will, therefore, help us to answer the questions when we realise what has been done elsewhere. It will be better, however, to omit the American results, as their "Blue-points" are quite a distinct species from the European oyster. When we speak of the oyster we generally mean *Ostrea edulis;* the common American oyster is *Ostrea Virginiana*, and differs totally from the European oyster so far as regards the reproductive organs. The latter has united in the same individual both ovaries and testes, though they are not functional at the same time, but in the American species every individual is either male or female. In the European oyster the development of the fertilised egg takes place within the mantle-chamber of the animal, while in the American *O. Virginiana* the fertilised eggs develop outside of the animal in the surrounding medium. While artificial fertilisation in the American species can take place, and the same methods may be applied as have been so successfully adopted for most of our ordinary food fishes, the habitat of the fecundated egg of the European oyster is so different in composition from the sea water, that it is impossible to apply the same methods as, for example, in the case of the salmon, if it be not impossible for the eggs to be even artificially fecundated and developed. It is plain, therefore, that different plans must be adopted from the ordinary piscicultural methods, and to the French is due the credit of the application of the new method on a large scale, and the great success in modern oyster-farming.

The supply of the French oyster beds had been decreasing. The late Emperor of the French was persuaded by M. Coste to carry out experiments with the view of increasing the oyster supply. M. Coste visited Lake Fusaro in 1858, where modern oyster culture was first practised in Europe. He was so struck with the successful attempts at oyster culture there, and of its adaptability to the French coast, that he instigated the French Government to take up the subject. The oyster, after living for some time within the mantle-chamber of the mother, is set free, and possesses an organ for swimming about in the water. Its free-swimming days do not last long, and the minute spat affixes itself to rocks, stones, shells, &c. The Fusaro method consists in supplying the spat with resting places in the shape of stones,

faggots, and branches. These collectors, which must be clean when placed in the water (and they should only be placed in the water a few days before the spat is ready for fixing), become covered with multitudes of young oysters.

When M. Coste was developing his plans, another Frenchman, M. Lebœuf, started the same system at Ile de Ré, off the coast of the Charente Inferieure. Though an unparalleled success has resulted from M. Coste's efforts, there were also failures; but now, after a lapse of thirty years, it is possible to form an accurate judgment of the successes and failures. The latter are quite cast into the shade, and the revenue derived by the French from their oyster fisheries, with the increase of capital which has been created in the industry, has a thousandfold justified the expenditure by the French Government at the commencement of the enterprise, and justifies the care and attention still bestowed on oyster cultivation. Taking the Bay of Arcachon alone, the quantity in stock at the end of 1887 was upwards of 400 millions of oysters.

There are two distinct branches in the industry—(1) the reproduction and collection of the spat; and (2) the fattening, and in some cases the greening, of the oyster. These are all cases of foreshore cultivation, but, in addition, dredging for oysters in the deeper waters is regularly carried on at some places. Taking the first branch, a stock of breeding oysters is kept by the *concessionnaires* on their grounds and by the Government for the benefit of the seamen of the Maritime Inscription on the natural banks *(bancs reservées)*. When, in June, the temperature of the water on the banks rises to 65°—72° Fahr., the oyster farmers place their collectors in the neighbourhood of the breeding oysters in a suitable position and arrangement for receiving the spat which is about to affix itself. In a very short time, if the collectors are in a favourable position as regards currents and eddies, they are soon covered by the young oyster growth. The most approved kind of collectors are roofing tiles, which are specially prepared in order that the spat may be easily and economically removed. The preparation of the tiles consists in coating them with a thin layer of lime and a thicker layer of mortar. But, besides mortar-coated tiles, planks of wood, stones, and clean shells are also used for the reception of the spat. In those places where the tiles cannot be laid on the ground owing to the great accumulation of mud, they are strung high on stakes driven into the mud.

After the lapse of some months, when the young oysters are fit to be removed, they are detached from the tiles and placed in *ambulances* or *caisses ostreophiles.* These are wire-work covered trays, and they protect the detached young which would otherwise fall a prey to crabs, starfish, whelks, and other enemies of the oyster. When the shells of the young oysters have increased in size and thickness, they are removed from the nursery, and placed in specially-prepared ponds or *claires* on the foreshore. In these *claires* they are regularly tended till they are sent away to the feeding, or fattening, or greening grounds. That, shortly, is what obtains in what I have called the first branch of oyster culture.

The conditions which are most favourable for the production and rearing of the spat are not the best for fattening operations, though it is possible that both branches may be carried on alongside of one another; but then the results are not the most conspicuously or financially successful. So, the young oysters are transported to a good feeding locality, often at a great distance from their birth-place, where they rapidly fill up and fatten. Most of the English natives are born in France or Holland, and are fattened at Whitstable or other beds in the south of England. To many epicures, the most delicious oyster is the green oyster, whose colour is due to the great number of the Diatom, *Navicula ostrearia*, in the mantle, gills, and liver of the mollusc. After a few months at the fattening or greening beds, the oysters are ready for market, and really fat oysters will always command a good price.

The Dutch system is closely modelled after the French, and its success is equally marked. In Zeeland, where oyster culture was begun about twenty years ago, the Dutch Government derive a revenue of £30,000 as yearly rental from the mollusc beds in the Schelde. To have created that revenue from a limited area of estuarine sea-bed within a quarter of a century is a tribute to the wisdom and energy of the Dutch Government and their tenants.

I have selected these instances of what two nations have been able to do in developing their oyster fisheries, and so increasing their national wealth, because I am personally acquainted with most of their ground and with their system. I might also cite the evidences of the activity of the United States of America, and their success in oyster breeding, but as their oyster is a

different species from the Scottish oyster, I will now deal with the question whether the Scottish oyster fishery can be resuscitated.

As to the *biological and physical conditions*, Scotland is well within the geographical area of distribution of the oyster. It is found from the North of Spain almost as far north as the polar circle in Norway, and from the West of Ireland eastwards to the Danish coast. It is particularly abundant on the coasts of the Gironde, Charente Inférieure, Brittany, the English Channel, and in the Schelde. Within the geographical area indicated it occurs also in large numbers at other places than those mentioned, and although it has not been recorded for others, still a more complete list of the fauna might reveal it at such localities. In this area it inhabits a wide zone, from the banks at Arcachon which every tide lays bare, to depths of 15 or 20 fathoms in the North Sea. Within these bathymetrical limits it lives gregariously, and so forms regular oyster beds. In Scotland the beds are remunerative at Loch Ryan and West Loch Tarbet, both of which are systematically fished. Both of these are shallow lochs, as are most other areas where oysters are obtained in great abundance, though this rule does not universally hold. The most productive beds are those with from half-a-fathom to five fathoms of water above them at the periods of lowest ebb tides.

As to the *character of the bottom* on which oysters live and thrive, that which is richest in food is one containing mud in its composition, and so the best oyster ground is one where there is a proportion of mud along with sand and shells. Although I have seen between Ile d'Oleron and the mouth of the Seudre 60,000 fattened oysters gathered during one tide from mud in which the gatherers sank almost to the knees, yet oysters can be deposited on such mud when the currents disturb it only very slightly; otherwise movable mud buries and kills this mollusc. In some of the Brittany estuaries special means have been adopted to combat the ravages of the great accumulation of mud there, by having the collecting tiles placed a considerable distance above the mud, and by mixing the foreshore mud with gravel, so as to harden the bottom, and convert it into a mixture not unlike concrete.

The *localities* in which oyster culture has been most successful are protected from great storms, and are generally *cul-de-sacs*, with an inflowing and outflowing tide. The tide is a great auxiliary to the ostreiculturist, as it brings within the reach of

the oyster not only fresh supplies of oxygen, but also hosts of Infusorians, Radiolarians, Desmids, Diatoms, and other food forms. It also carries the spermatozöids from one oyster to fecundate the eggs borne in the ripe female, or, I should rather say, to the oyster in which the female sexual products are ripe. The tide further creates the currents and eddies that are so helpful to the spat migrating to suitable resting-places.

Many places in Scotland, therefore, so far as regards the nature of bottom, shelter, and currents, are suitable for oyster farming. There are, however, two other factors that must be taken into account before we can arrive at a judicial conclusion on this subject—namely, the temperature and specific gravity or saltness of the water.

When we consider the subject of *temperature*, it will help us, if we keep in view the fact that oyster culture, as most scientifically practised, has two branches—(1) the production and rearing of spat, and (2) the rearing and fattening of the oyster. For the production of spat in abundance on the French and Dutch coasts, a temperature of 65° Fahr. is said to be necessary; but if this—the production of spat—means simply the presence of white or black spat within the mantle-chamber of the oyster, then I have found in our Scottish waters oysters, where the temperature had not attained so high a point, which had, nevertheless, an abundance of white spat in some, and in others of black spat. It would perhaps be more correct to say, that this temperature is requisite for the life and growth of the spat between the time when it is shed from the mother and the fixation of it on its resting-place. After a certain period, the young can withstand a much lower temperature, and the mature forms live in water, as, for example, on the German coast, where the temperature is as low as 2° Fahr. below the freezing point. So long as a depth of water at this temperature covers the oysters, no harm accrues to them, but, if the oysters are left on the dry banks at low tide, so low a temperature is apt to prove fatal. When young oysters are about to be transported to other grounds at a distance, they undergo a process of education. The *ambulances* containing them are removed gradually higher up the shore, and are bare during each tide from one hour at the beginning of the process, to four or five hours at the end. Then they learn how to keep the shells shut for a considerable time, and so withstand the cold and other conditions of transport. The spatting time of the oyster occurs in

summer, from June onwards, and if the water is continually warm during the summer months, it is reckoned a good oyster year, even though the cold of winter may reach almost to the temperature when ice is formed on salt water.

With reference to the *specific gravity or density of water*, this is perhaps most intelligible when expressed in terms of the percentage of salts present. Here, as in the condition of temperature, a difference is manifest between breeding and fattening. Oysters will fatten best where the water is not too salt, or is but slightly brackish. They breed, however, only in sea-water, containing at least three per cent. of salts. Hence it is that a good fattening ground is not a good breeding-place, and, though oysters will fatten on breeding-grounds, the oyster farmer finds that the more rapid improvement in taste, flavour, and bulk amply compensates him for the expense and death-rate in transport.

Our coast certainly furnishes us with water which meets the requirements of specific gravity or saltness, but the conditions of temperature are not so favourable for breeding as in France and Holland, where the sea-water is warmer in summer than in Scotland. The Fishery Board for Scotland is having daily temperatures taken at selected places along our coast, and the record of these will be interesting, and will furnish a guide to prospective cultivators of oysters. At a few places on the West Coast I obtained specimens of young native oysters and of oysters of ages from one year up to five or six years, if not older. This shows at least that individuals have been spatting, though the proportion of eggs evacuated to oysters reaching the adult stage is said by authorities to be very small; and it must be very much less on our coasts, where collectors are things of the future, than in France and Holland, where every effort is made to capture the spat on the tiles and other collectors. Whether we can rear much spat by adopting the most approved appliances for their reception, is a question for the future of oyster farmers in this country. That we can feed and fatten oysters in great quantities I make no doubt. At La Tremblade, in the Charente Inférieure, one proprietor of oyster fattening *parcs* informed me that, during the fattening season—which only extended over four to six months—his profits amounted to one hundred per cent. on the price which he had paid for the oysters. But, though we cannot look for such returns in this country, the higher price which oysters for the table command offers great inducements for a trial.

I have not touched on the vexed question of ownership. The Fishery Board for Scotland, following the example of the Board of Trade—whose fishery functions, so far as regards Scotland, have been transferred to it,—states that it will favourably consider applications for ground for oyster culture. It has already granted Fishery Orders for West Loch Tarbert and for Loch Swen. The former has been successful, and I hope the latter will also be successful. There is plenty of ground suitable for carrying on the second branch of oyster culture in Scotland, and by a judicious crossing of Scottish natives with Dutch or North French oysters, it might be possible in a warm summer to obtain spat in greater quantity than at present falls on our ground. It is, certainly, not because the climatal conditions have altered in Scotland within the last century that the supply of spat has decreased. Wherever the numbers in a community such as a bed of oysters become lessened, the remaining members are not so able to withstand the attacks of enemies; and even though the females produce eggs by the million, the proportion of those that escape all dangers, and reach maturity, is exceedingly small. Where no means are taken to protect the oysters by the cleaning of the beds, the destruction of enemies, and the furnishing of suitable resting-places for the spat, and where the stock on the ground is allowed to fall below a certain point, only destruction of the bed is courted.

It will be evident from what has been stated, that it is quite possible to put a stop to the further diminution of our oyster supply by adopting a more enlightened policy than has hitherto been pursued in Scotland. Not only so, but the revenues from our oyster fisheries may be considerably increased. There are two parties who each advocate a different policy as to the method of accomplishing this. One party favours State intervention, and claims to have measures adopted similar to those taken by the late Emperor of the French and his government. The large sums of money spent by the French authorities were undoubtedly the turning-point in oyster culture in France, and the result has amply justified the means employed. The initiation of French oyster culture was due to this, but the wise regulations which have been enforced with care, and under the supervision of their skilled scientific advisers, have contributed to the continuance of prosperity in their oyster breeding. One leading motive for the attention which the French authorities give to oyster culture is to find a

source of income to their time-expired marine conscripts, and the result is contentment amongst this class, who are always at hand for further duty in the nation's cause. The advocates of individual enterprise rather than of State intervention and subsidy, allege that individual is preferable to State enterprise. It is for them to make their advocacy a reality. Suitable ground is to be had on the western seaboard for the rearing and fattening of oysters. We only await some of the enterprise shown in other commercial undertakings to be transferred to the question of oysters, to hail an increase in our supply of native oysters, and to make oyster culture in Scotland a success.

VII.—*An Art Museum: Its Structural Requirements.* By JAMES PATON, F.L.S., Curator of Kelvingrove Museum and Corporation Galleries, Glasgow.

[Read before the Architectural Section of the Society, 16th February, 1891.]

LET my first word to the Architectural Section of the Philosophical Society be an assurance that I have not come here this evening with the presumptuous purpose of teaching architects their profession. A client does not, I presume, do that when he entrusts a professional man with the erection of a house, which he stipulates shall be suitable to his condition and to the number of his family. The point of view from which I wish to approach you is precisely that of the client. I shall endeavour to formulate a few of the simple necessities of the structure which I earnestly desire to see you engaged in designing. The formulation of these may be of some use to you—as for myself, I find it, after many years of general observation, of great value in the way of giving precision and definite form to notions which, until committed to paper, are only vague and imperfect. What I have to say may appear to all who have given consideration to this subject, simple and obvious enough, yet, elementary as the statements are, they are not without value, and they cannot be too strongly and frequently borne in upon the minds of architects. For we are not without costly and disastrous examples of the evil of allowing an architect an entirely free hand in dealing with the erection of an Art Museum. The most recent instance we have of a great museum so dealt with is that opened within the past two years at Amsterdam for the accommodation of the world-famous collection in that city. That institution, the Rijks Museum, was finished at an expense of about a million sterling by Mr. Cuipers, who has the reputation of being the foremost of Dutch architects. The building for which Mr Cuipers is responsible is, externally, a triumph of Gothic architecture; internally, it leaves very much to be desired. It contains a fine confusion of nearly three hundred

apartments, so that the visitor never knows where he was, is, or shall be. It is resplendent in colour and ornament, where it ought to be severely restrained and reserved. Its lighting errs in both extremes—in excess as well as in deficiency; and while it was intended to enshrine and specially glorify the famous "Night Watch" of Rembrandt, it has so disposed that marvellous work that people who came from every corner of the world to admire it in the modest old Trippenhuis can scarcely recognise the masterpiece in its new environment.

The generous and judicious decision of the Town Council of Glasgow has placed at the disposal of the coming architect of its Art Museum a site which, for spaciousness and surrounding attractions, can be equalled in few great cities. With the ample means which public spirit and the proverbial liberality of Glasgow will surely soon provide, the architect will have an opportunity such as seldom occurs, even in the life of an architectural artist, of providing a monument of enduring honour at once for himself and the city: for treasures of Art should be enshrined within a fitting casket. With a slight travesty of the advice of Polonius to his son, we may say:—

> "Costly thy *building* as thy purse can buy,
> But not expressed in fancy—rich, not gaudy."

As to the style of architecture best suited for the building, I care not to give any opinion, but it should be so planned that all parts shall be well and fully lighted. It, moreover, should be capable of being indefinitely added to without structural alterations, in a simple, symmetrical, and orderly manner: for, to begin with buildings on a great scale is a thing not desirable; and vast halls and galleries encourage the acquisition and retention of objects of indifferent merit, which add nothing to the real value of a museum. On the contrary, as "evil communications corrupt good manners," the presence of inferior specimens of art in a museum, reflects suspicion on, and detracts from the importance and dignity of, what is really good and meritorious. The value of an Art Museum is therefore not to be reckoned by the square yards of wall which are occupied by its pictures, the cubic contents of its cases, or the total number of objects stored or shown within it; but rather by the rigorous selection of such works only as possess the highest merit, and of specimens which most impressively teach the lessons they are meant to convey. A Museum of Art as it grows in age should grow in wisdom. It should increase and become richer—

not by mere expansion, but by a relentless weeding-out of the halt, the lame, and the infirm; and I may at once confess, that this process of weeding might be applied to our Glasgow collections with beneficial effect.

When we come to deal with the interior of the structure, it is my duty to warn the architect that he should maintain a reticence and self-restraint, which amount almost to self-obliteration. Like the king's daughter, the palace of Art should be glorious within, but that should be with the radiance of the treasures it is built to display, and not with the cunning device and decorative resource of the architect, however great and varied these may be. The walls are not there for chromatic display, nor for rich panelling; and pillared and pilastered bays and recesses are equally out of place. A simple, well-ordered plan, so that visitors may always know where they are, and where they have been, with ready access to all stages and departments, to enable them to return with ease to any department they may wish to revisit, are the essential and meritorious features of internal design. For reasons I shall allude to further on, the entrance to picture galleries should, as far as possible, be removed from direct communication with the external air. That, fortunately, is accomplished in all galleries built according to modern requirements, by placing the picture saloons on the upper floor of the building, leaving the lower stages for the industrial Art collections. The plan—originated, I believe, in the constructon of the Dresden Gallery—of disposing large pictures in spacious roof-lighted saloons, with cabinets or moderate sized apartments running alongside and side-lighted from the north, possesses great merit when a large and mixed collection of pictures has to be dealt with. In this way, what is termed sympathetic grouping can be attained, and the smaller pictures are so placed that they can be studied with comfort and ease. This plan has since been followed in the building of the old and new Pinakotheks, in Munich, in the Stockholm Museum, and in the recent Continental Galleries.

Dealing with what the interior of the erection should embrace, it is, to my thinking, an unfortunate circumstance that the habit has arisen of speaking of an Art Gallery as something apart and distinct from a Museum of Art. Art is one and indivisible; and, just as Wordsworth boldly proclaimed that into whatever region the intellect of man can penetrate, even were that the differential calculus, the transfusing imagination of the poet should be ready

to follow, so should the artist be prepared to say that whatever the formative skill of the artisan can fashion and fit together he can adorn and glorify. Equally with the philosopher and the poet, the motto of the artist should be "*Homo sum; humani nihil a me alienum puto.*" His sphere and function do not fall short of this; and he does not rise to the full conception of his place and power when he limits the field over which it is his privilege to range. And at the present day there are not wanting healthy signs of a desire and resolution to reassert the extended boundaries of the kingdom of Art. In mediæval times the great artist might be, and often was, great equally in architecture, in sculpture, in decoration, in metal-working, and even in the fashioning of jewellery; and in the case of the all-embracing genius Leonardo da Vinci the artist did not scorn to be also a skilled mechanician and a military engineer. It is not given to many to excel in all these departments—to be able to express themselves perfectly in so many different media; but let not the oil-painter on that account despise the worker in copper and brass, nor the sculptor deny the name of artist to the man whose skill in colour and design deserves the name, though it is expressed only on the surface of pottery or wall-paper. Therefore we shall, if you please, look upon Art in its more comprehensive aspect, and thus we use the term Art Museum for the institution which we imagine. And as we cannot limit the sphere of the artist's activity, so it is contrary to sound principles to limit the scope of an Art Museum. Every product of the heavens above, of the earth beneath, and of the waters under the earth, whatever the hand of man has cunningly fashioned, and moulded to his use or for the gratification of his taste, in all times and in all climes, will find appropriate place in such a comprehensive museum.

But our Art Museum cannot be formed according to such general and comprehensive conceptions alone. It must have purpose, method, order, and classification; and, moreover, it must have its scientific and general education as well as its art point of view. Within our Art Museum we therefore must provide, in the first place, suitable and ample picture galleries, in which oil paintings, water-colour drawings, and pictorial works in other media, engravings and etchings, photographic reproductions of great masterpieces of art can be properly conserved and displayed. An adequate provision for sculpture must also be made, having space to display a historical collection of casts of the great works

of the ancient and mediæval masters, and of the early Celtic sculptors of our own country, as well as modern productions. Then, in separate saloons, halls, and galleries, provision must be made for the collection of antiquities or historical art, for the ethnographical collections which illustrate the art instincts and abilities of the primitive races, tribes, and nations. After these, but not less important, have to be considered and accommodated the technological arts, or art in its relation to industry—the collections of pottery, glass, metal-working in all its phases, carving in wood, stone, ivory, and other substances which can be so treated; and the textile arts, embracing weaving, bleaching, dyeing, calico-printing, tapestry, and lace-making. Apart from these, and yet not to be excluded, nor treated as being beyond the scope of an Art Museum, are to be considered and dealt with the constructive collections, illustrating building, shipbuilding, engineering, and the physical and chemical sciences with which these and other industries stand related. We must also take into consideration the Biological Sciences—Natural History and Botany, which might also find a fitting place in our Art Museum; but inasmuch as these subjects are more strictly and purely scientific than the others, they can, with least harm to the whole, be separated and treated by themselves. Were the space in Kelvingrove Museum free for the Natural History collections alone, that would, for the present, afford accommodation sufficient for the display of an instructive type collection of the life-forms at present existing, and of the fossil remains, the prototypes from which they have been evolved. In that building could also properly be displayed a classified series of the rocks and mineral forms of which is built up the solid crust of the globe we inhabit.

These, in brief outline, are the principal divisions and sections for which accommodation is required in the galleries and halls of the Art Museum. It may not be out of place here to note that a museum does not consist of that series of public apartments alone. There must be provided dry and well-lit store-rooms, and offices of various kinds, besides one or two studios in which artists, and men of science engaged in special work or investigation, may be accommodated in quietness while examining, comparing, or copying objects temporarily withdrawn from the collections. A library, also, of standard and select works in Art and Science, to which the public should have access for reference, is an indispensable feature of an Art Museum. Either within the building which

we have sketched, or placed in immediate proximity to it, I consider it essential that at least one official residence should be provided.

Coming now to the special internal requirements of the Art Galleries, I naturally bear in mind, first and foremost, that my own function is that of a curator, one whose office is to care for and preserve. The primary duty of such an official is to see that the art property in his charge, the art inheritance of the ages, is handed down to generations yet to come, as far as possible, in no worse condition than that in which he found it. With many art objects this is a matter of little difficulty; with pictures, on the other hand, it is a delicate and, in some cases, almost impossible task. Oil paintings are principally executed on wood panels, or on prepared canvas, and but rarely on sheets of copper. The greater proportion of the works of the early masters were painted on wood, prepared and jointed with great care, and covered with a painting surface consisting of a gesso of fine plaster of Paris and size. On that surface the artist painted his picture, using various pigments which behave differently towards the oil or other medium in which they are worked, and which have different degrees of stability in light or darkness. At various periods these pictures may have been coated with one or many layers of varnish, and the object to be conserved consists thus of layers of wood, plaster, paints, and varnish, all variously affected by heat, cold, moist air, or an over-dry atmosphere. It is the same with pictures painted on canvas, which absorbs moisture, and expands or contracts under the influence of varying atmospheric conditions. Heat, moisture, and light thus affect our pictures, and to these we must add a fourth agent, unfortunately in Glasgow the most destructive of all—impure air; and these agencies have to be primarily considered in the construction of galleries for the conservation of pictures. The injuries which accrue to a picture from these causes of deterioration are not such as can be demonstrated in a lecture experiment; they are subtle and slow of growth, and, when stated and expounded, they may appear to some so trivial and unimportant as scarcely to merit notice; but it is just against such slow and insidious, but unceasing, deterioration that we have to be on our guard in dealing with objects to be preserved, if possible, to all time. Sudden ailments and rapidly devastating action give immediate notice of themselves, but the influences which we have to fear are those which it may take a generation to manifest their evil work.

How great and sudden fluctuations of temperature act in the unequal expansion and contraction of paint, panel, canvas, and stretcher, and in the chilling of varnish, I do not need to wait to tell you. The absorption of moisture by wood and canvas, and the variations in the amount of water retained by these substances partly also depend on the fluctuation of temperature, and partly on the amount of moisture with which the atmosphere around them is loaded. The accumulated evil influences of such variations soon amount to perceptible deterioration in the case of oil paintings, while for water colours it produces swift destruction. Evil as is the influence of damp, it is not more hurtful than the sudden exposure of a moisture-impregnated panel or canvas to a current of warm and, consequently, partly-dried air, or to the direct influence of radiant heat. A picture in such circumstances contracts and expands, if not visibly, still at least measurably; and the fissures, blisters, and broken patches which are to be seen in many ancient and in some modern pictures are due to such influences.

Into the question of light and its effect on pigments I do not require to enter at this time. The subject has been much debated and investigated; but, as regards oil paintings at least, universal experience has proved that the light in which a picture may be best seen is that in which it may be kept with greatest safety. Painters use many colours, some of which are practically unalterable, a few mellow and soften by age, with the result that they yield tones more harmonious than the artist originally conceived; and some, principally of modern introduction, are altogether fugitive and unreliable. The permanency of an oil painting is largely dependent on the right choice of pigments, and the use of proper technical methods by the artist himself. A picture is painted to be seen; to be seen it has to be exposed to the light, and, if it fails to stand that exposure, it lacks the qualities of an enduring work of art. Oil pictures kept in a too-dark situation darken in the high lights in which flake white is used, and the varnish also becomes opaque. A picture in such plight will clear and brighten wonderfully by being brought into and kept in a clear light for some time. Certain rays of light it is known possess much greater chemical activity than others, and these, it has been demonstrated, are the most effective in the fading of colours. But it has not been found practicable to sift out these rays in any gallery by the interposition of screens of tinted glass or other means.

Artificial light, such as gas, is no more injurious as light to works of art than diffused daylight, nor need the heat given off by the consumption of gas be hurtful, provided there is sufficient distance between the burner and the pictures, and that the products of combustion be not allowed to diffuse in the apartment. Imperfect combustion, however, in ordinary open burners yields soot, water, carbonic acid, and sulphurous acid, all of which, if diffused within the gallery, react with destructive effect on painted surfaces, and on the substratum which supports the painting. The possible escape of coal gas also within a picture gallery, with the sulphuretted hydrogen and other contaminations from which it is never free, is most detrimental in its influence. Fortunately, the incandescent electric light affords a means of illuminating a picture gallery, practically free from every objection which can be urged against any other artificial system of lighting.

The question of contamination from within brings us to the consideration of our last destructive influence—the impurities of the atmosphere. Few people and very few collections of pictures are fated to live in a more filthy atmosphere of dust, soot, acid vapours, and sulphur compounds than it is our fate to endure in this grimy city. We may look forward, as the prophetic writer in "Looking Backward" has done, to a time when our streams shall be as pure as the mountain burn which scars the sides of Ben-y-gloe, and when our air shall be as fresh as the Atlantic breeze when it first strikes the sides of Ben Nevis. But in the meantime we have to do with widely different conditions. We are literally bathed in soot, and with our moist atmosphere that substance acquires such a finely-adhesive quality that the gentlest current carries and deposits a veil of black on every substance with which it comes into contact. And air, soot, and dirt bring with them, and deposit more secretly, but even with deadlier influence, acid vapours with which the atmosphere is plentifully charged. Soot, dust particles, and mineral acids—the most destructive influences apart from mechanical and manual violence—are the chief foes against which we have to fight. They are deleterious to human life, and to the life of a picture they are fatal.

Here, then, is the problem for the architect: How to maintain an equable temperature; pure air, neither laden with, nor deficient in, moisture; and proper daylight and artificial illumination in the galleries devoted to the exhibition of pictures. The

difficulties of the problem are not lessened when he has to take into account the fact that at one time the galleries may be crowded with an ever-stirring mass of visitors, and again entirely tenantless. Clearly this is a case not for perfect solution, but for doing the best possible in the circumstances; and, fortunately, the problems with which the architect has to deal have received much investigation and attention in recent years. It is obvious that he must have the means of artificial heating under perfect control; and in connection with the heating the system of ventilation must go hand in hand. The ventilation of such galleries can only be efficiently controlled by mechanical agency. The air to be admitted should enter only by the permission of the engineer, and by channels appointed for it, in which, after being duly attempered for heat, it should be sifted from all mechanical impurities, and as far as possible from acid vapours. To permit of this control of the atmosphere within the galleries, it is obvious that the entrances to them should be, as far as possible, cut off from direct communication with the external air. Concerning lighting, also, the desirable ideal is likewise unattainable. The ideal condition is that the galleries should be uniformly flooded, and the pictures illumined with a diffused, steady, and unvarying light. What we too often get is the reflection of window or roof-light from the glazed or varnished surface of the picture, giving it the effect more of a mirror than a work of art. Or, avoiding such glitter, the picture is so slanted forward that the light merely grazes its surface, does not penetrate its shadows, and where the surface is loaded with a heavy impasto it throws strong black lines on masses which are intended to reflect high lights only. So here again we have to accept a compromise between lights so high that they graze and skim the surface of our pictures, or so low that they reflect themselves directly into the eye of the observer. It is desirable to admit the light at as low a level as possible, and to avoid glitter it is only necessary that pictures should not be piled above each other in the ridiculous manner which demands the use of a telescope to see them properly. Of the advantage of disposing cabinet pictures in moderate-sized apartments lit from the side I have already spoken. Of roof-lighted galleries the modest little saloons erected at South Kensington Museum for the "Sheepshanks Collection," which form the nucleus of the permanent buildings there, appear most satisfactorily to answer the requirements of lighting. These apartments measure 46 feet in length

by 20 in breadth; the light is admitted to them from the roof at the height of about 21 feet above the floor; and the total amount of light opening is equal to exactly one-half of the floor area. Thus, the floor measuring 46 feet by 20 feet gives an area of 920 feet, and the glazing in the roof, 46 feet by 10 feet, equals 460 feet.

Hitherto I have been dealing with the strict necessities of an Art Museum alone, but before concluding I may be allowed to allude to the larger scheme of which this Museum forms the primary and central part. In an eloquent address, informed with glowing enthusiasm for the social welfare of our brother citizens, my Chairman, Bailie Crawford, a few weeks ago, sketched out an institution which, while ministering to the taste of the most cultured, presented recreative, innocent, and elevating attractions to the humble and untutored. In this I am entirely at one with Bailie Crawford, for the Museum is of the people and for the people, for rich and poor, high and low. More indeed it will minister to the humble and needy than to the wealthy, who possess their own pictures, their own sumptuous books, and their own art objects. Art collections the people can only possess in common, and the Museum they should learn to regard as their own property, placed within their own park, and surrounded with their own flower gardens. Therefore I say it becomes a duty to render such an institution cheerful, bright, and gay, and to attract the people into it by every honourable device. As a powerful attractive agent, and, I may say, equally on its own account, music should have a place in the institituon; and a music hall in which an orchestra can perform, or other music be rendered, is a most important adjunct of the Museum scheme. This is not altogether a new departure; even were it so, no less reason is there why it should not be undertaken. We have learned much from other cities and institutions, and why should we not repay them by teaching something to those who are to follow us? Around the arcades, and against the walls of this hall, the collections of sculpture might be suitably disposed, partly as ornament, partly for instructive purposes. Such a hall, adorned with shrubs and flowering plants, would form a suitable centre for the accommodation and refreshment of a vast number on the occasion of any public festival or rejoicing. The vestibule or entrance hall of the building, I also agree with Bailie Crawford, should be spacious, and fitted to receive public memorials, in the form of statuary,

mural tablet, or other permanent record of the names and memory of those whom the city delights to honour. Without altogether agreeing with Mr. T. M. Healy that all monuments in public streets are public nuisances, it does appear to one that a statue may be more fittingly placed, and the man's memory more decently held in respect, by enshrinement within a local Walhalla, than by exposing such memorials to the pitiless pelting of our soot-laden blasts.

Of other institutions which should be associated and placed in direct communication with such a repository of art, I place in the first line a School of Art. Without the resources of an Art Museum, a School of Art can have at best but a hampered and hindered usefulness, while the Museum dissociated from the School fails in one of its most important educational influences. As part of the necessary equipment of both, there should be provided a lecture theatre in which an audience of at least 600 persons could be accommodated. Finally, I see no reason why in the end the Technical College of Glasgow should not also find its fitting home in connection with, or in immediate proximity to, the Museum; so that that institution also would be able to draw from the resources of the collections for lecture illustrations. In this way we may conceive of Kelvingrove Park becoming ultimately the centre of artistic and technical instruction in the city, the home of the Glasgow Institutions of Science and Art, the source of intellectual and æsthetic cultivation, for the whole mass of the citizens. With such a cluster of buildings planted on the hither side of the Kelvin, with our noble University crowning the height beyond, we should have within a little compass a series of institutions on which Glasgow might indeed look with pure gratification and pride.

VIII.—*The Relations of Thomas Carlyle to Political Economy.* By JAMES BONAR, M.A., LL.D., London.

[A Communication from the Economic Science Section, and read before the Society, 10th December, 1890.]

CONCERNING most of our great authors, such a subject would be unmeaning, or even ridiculous. It would be a very idle task to discuss the relations of Wordsworth, Byron, or Scott to political economy. Literature (in the narrow sense, as one of the Fine Arts) has cultivated no very intimate relations with economical study, and perhaps could hardly have done so with any profit to either party. If it is rare for a philosophical treatise to pass into literature, it is still rarer for an economical one. In the days before prose, a poem on the "Works and the Days" might (it is true) be the only possible shape which an economical book could take; and even in later times an economist who feared to seem too confident in the truth of his own reasonings might put them into the form of a story and call it "Utopia." In France, before the Revolution, there were economical writings which it would be hard to exclude from the category of general literature.* But, on the whole, literature and economics have drawn apart. Modern "Tales of Political Economy" certainly do not belong to literature; and economic novels (like "Looking Backward") have even less of the literary character than most historical novels have of history, or the romances of Jules Verne of science. From the nature of the two, Economics and Literature must go apart. The former is bound to be *analysing* something and *proving* something, or else it fails in its duty. But a literature that fell into these habits would hardly be literature. We can well understand, then, that Carlyle, as a literary artist, could have no liking for economics;—why then (it will be asked) should we examine his relations to it? Will it not be an attempt to find out a man's answers to questions that he has never asked himself? It is not so. Among men of letters Carlyle is in one respect like Southey and Coleridge: he

* *E.g.*, "L'homme aux quarante écus."

is preacher as well as artist; and, unlike them, he is never the artist without being the preacher, or the preacher without being the artist. He has a burden like that of Isaiah or Ezekiel, and not merely a love of breathing thoughts and burning words for their own sake. Now, to the preacher and prophet, all that influences the "Spirit of the Age" and gives it character is of interest. And it so happened that in Carlyle's days the political economists had a large share of this influence; they had more weight with public opinion than they have ever had in our generation, or will perhaps ever have again. Their teachings (or what he understood to be their teachings) could not fail to come under his ken; and the things which he said about themselves (and especially the epithets which he succeeded in fixing on them and their studies) undoubtedly told against them then, and are still alive now to create prejudice against their successors wherever Carlyle's books are read.

In the early part of Carlyle's life (even before the comparatively late period at which he entered on authorship as his vocation), and especially at the time when he was becoming conscious of his powers—say, about the year 1820,—political economy had not been very long in existence as a separate study; and perhaps for that very reason its teachers, especially in this country, were very emphatic in asserting its scientific character. The "nature and causes of the wealth of nations" were then for the first time counted a large enough subject to require the special devotion of a separate class of scientific students. Before Adam Smith (at least in our own country), it had been supposed to be open to every man of ordinary understanding to form conclusions simply as a casual result of his own experience and mother wit, about the subjects we now call economical. Henceforward, only he who had made patient and thorough investigation in the clear, dry light of science, as opposed to the misty light of nature, was to be qualified to have an opinion on these matters. One more department of thought was claimed for science and reclaimed from common opinion. In our country it was Adam Smith that secured this result. The conclusions of that eminent economist made a deep impression on the public mind. They were, perhaps too readily, taken up and applied to *politics*, without the reasonings that led to them and the Scotch caution that had led Adam Smith to fence them about with all manner of careful reservations. Such a saying as—"all restrictions being taken away, the simple system of natural liberty establishes itself

of its own accord," seemed clear in itself and easy to apply, for it seemed to fit the tendencies of an age of revolution and reform. So it was that such statesmen as Pitt, Huskisson, Peel, and Cobden appealed to the authority of Adam Smith when they endeavoured to remove restrictions on foreign trade, and on home industry, and on liberty of combination. And the successors of Adam Smith were also a political power. Without Ricardo there would have been no Bank Charter Act, and without Malthus no reform of the English Poor Law. It must be added that the appeal to the "system of natural liberty establishing itself of its own accord," was in some quarters used as a weapon against Factory Acts.

In the early part of this century the economists were strong, for another reason. They seemed to be the only people that had a political philosophy, almost the only class of political reformers that seemed to know their own minds. A man like James Mill, taking up Bentham's principle that a man's chief end was his own happiness, and a people's, the "greatest happiness of the greatest number," found in it a meeting point for economics and politics; and seemed to have the strength that comes from a creed of a few articles simple enough to be intelligible to every body, and stating a programme apparently adequate for all purposes. As Carlyle writes in his "Life" (vol. II., p. 90, date 1830), which may be taken as a commentary on his published writings:—"The Utilitarians *have* logical machinery, and do grind fiercely and potently on their own foundations, whereas the Whigs have no foundation, but must stick up their handmills or even peppermills on what fixture they can come at, and then grind as it pleases heaven." Nevertheless, even in his most radical days, Carlyle was no lover of these utilitarians, and, as he associated political economy with utilitarianism, the former was included by him with the latter under one common condemnation. The first economist he met in the flesh (at Edinburgh, in 1823) was John Ramsay MacCulloch, Editor of the *Scotsman*, who eyed him, he says, with suspicion and distrust, as if he feared Carlyle had come to spy out the nakedness of his land (Life, vol. I., pp. 175-6). Ten years later, when he again saw MacCulloch (in London, October, 1834—Life, vol. II., p. 468), he confessed that, though MacCulloch was "a hempen man," it was "*genuine* hemp." I do not think we can be wrong in identifying *M·Croudy*—who is Carlyle's usual personification of political economy — with

MacCulloch; there was no other Scottish economist to furnish the patronymic. To Dr. Chalmers he was always respectful, and with John Mill he was in familiar friendship till they parted on the "Nigger Question" (end of 1849).* It was not from mere prejudice against particular persons that Carlyle took a brief for the other side—it was from principle, and from his own personal bent which was against "victorious analysis" and minute scientific inquiry in any form. Darwin (see his Life by his Son, vol. I., pp. 77-8, date 1839-42) says, after seeing him:—"As far as I could judge, I never met a man with a mind so ill adapted for scientific research." Though he had mathematical talent, he had no love of physical science; and it seems to be true that he never showed, *except* in biography, and in history, which to him meant biography, that unwearied patience over minute details which is essential to a scientific inquirer. There is, therefore, a presumption that his dicta about any kind of science are Jove's thunderbolts without Jove's omniscience. Fortifying ourselves with this comfort, we may now look at some of his judgments on the professors of that study to which an important Section of the Glasgow Philosophical Society is devoted. I must say again that I take any words of his recorded in the "Life" (or in any other indirect way) as a *commentary* merely on his published utterances; it seems unfair to go to them for any opinion not stated in Carlyle's books. "Your freest utterances (he says himself in 'Past and Present,' bk. III., ch. v.) are not by any means always the best; they are the worst rather." And it is right to take him at his word. When he prepared his books for the press, he was careful to make his expressions represent his matured judgment at the time of writing; but his *spoken* words, or his hasty thoughts jotted down in his diary, were the unchastened feelings of the passing moment, valuable only as showing what feelings lay under his thoughts at the time such and such a book was written.

Let us begin with one of these outbursts—the date is Craigenputtoch, between 1820 and 1830 (Life, vol. II., p. 78):—"Is it true that of all the quacks that ever quacked (boasting themselves to be somebody) in any age of the world, the political economists of this age are for their intrinsic size the loudest? Mercy on us, what a quack-quacking, and their egg, even if not a *wind* one, is of value simply one halfpenny. Their whole philosophy (!) is an arithmetical

* Life, vol. IV. (II. of Later Life), p. 26.

computation . . . , and if this were right (which it scarcely ever is, for they miss this or the other item, as they will, and must return to practice and take the low *posteriori* road after all), the question of money-making, even of national money-making, is not a high but a low one;—as they treat it, among the highest. Could they tell us how wealth is and should be distributed, it were something; but they do not attempt it."

That is one definite objection at least; political economists in his day confine their attention to the production and exchange of wealth, and say too little about the distribution of it. This was largely true. But he goes on:—"Political Philosophy? Political Philosophy should be a scientific revelation of the whole secret mechanism whereby men cohere together in society: should tell us what is meant by 'country' (*patria*), by what causes men are happy, moral, religious, or the contrary. Instead of all which it tells us how 'flannel jackets' are exchanged for 'pork hams,' and speaks much about 'the land last taken into cultivation.' They are the hodmen of the intellectual edifice, who have got upon the wall and will insist on building as if they were masons" (*ib.*). This is in reality a second objection: political economy in his opinion claimed to be a political philosophy, and had no right to do anything of the kind.* This was really because there was no political philosophy except the utilitarian before the English public at that time, and the economists were usually utilitarians. How far such charges could be maintained even against the utilitarians I will not say; but, when Carlyle foresaw that the utilitarians were soon to "pass away with a great noise" (Life, vol. II., p 79), he might have allowed that after that event political economy would have something to say for itself, when it had ceased to be associated with utilitarianism. It would not *miss* its associate. To many of us it seems a positive hindrance to the fair fame of political economy now, that its professors still talk of a "calculus of pleasures and pains," as if that were the foundation on which all economical theory must rest. If the economist is no longer supposed to assume that all men act only from self-interest in the narrowest sense, why should he be supposed to measure only "pleasures" and "pains"? Human interests (as Carlyle quite rightly protested) are not rightly or fully described in terms of pleasure and pain (unless these words are so twisted as to mean

* The attempt of physical science to make itself a philosophy or to speculate on the origin of things might be taken as analogous.

what does not belong to them in ordinary speech at all). The economist measures the effects of certain motives and certain conduct, in relation to a particular subject—namely, the material good things of this life, and without any necessary concern with the motives and conduct themselves as a subject of psychology.

Men have wants and satisfy them by material means; the outward acts and the intentions and aims they indicate are of economical concern. The relation of the motives to the acts, and the relation of human reason to human action, are, no doubt, of the highest concern to the moral philosopher, but not to the economist as an economist. He may have opinions about them because he may be philosophically minded and study them; or, without any detriment to his economics, he may not be inclined to go beyond them as they stand, in which case he should not adopt the language and conclusions of a particular philosophical theory. Yet this was what Malthus, Ricardo, James Mill, and Jevons did in their time; I fear the like has been done in our own time. The result is to prejudice people who are not utilitarians against a study which they naturally think must be bound up with the particular psychology and ethics of utilitarianism. Something like this may have happened in the case of Carlyle.

When Carlyle found political economy in neutral surroundings, as at the Court of old Frederick William, father of Frederick the Great, it is astonishing how respectfully he speaks of it. (See Fred. the Gt., bk. IV., ch. iii., &c.) And the commonplaces which lie on the threshold of all social economical studies, were to him a delight and an admiration. His praises of work and thrift (*e.g.*, Past and Present, bk. III., ch. iv.) have even found their way into School Reading Books. But the regard for these virtues, which he shares with Solomon and John Stuart Mill (to say nothing of lesser men), was owing, like Solomon's, rather to a fondness for moralising than to any bias in the direction of political economy. These virtues, in fact, seem specially in place in that department of study, only because they and their opposites have a specially large part to play among the causes of the effects considered by economists. But the economist, as economist, does not go into these causes; he looks to their effects. So is it with the achievements of industrial pioneers like the "rugged Brindley" * who made the canals, and the men of Cromwell's country who drained the fens, and the practical

* "Past and Present," bk. III., ch. v.

English generally, "whose epic poem is written on the earth's surface," and who, being stupidest in speech, are wisest in action. So with the wonderful works of steam-engine, power-loom, printing press,* and (broadly speaking) tools and machines,† which help man to conquer nature. These are rather technical than economical facts. They are concerned with the adaptation of particular material inventions to the causing of certain results which may or may not have anything to do with the riches and industrial resources of men; they may have to do with the killing of men and the wasting of their resources in the speediest manner, or simply with the killing of time and the wasting of intellect. The economic aspect of a rifle, for example, is very different from the technical; technically it is an instrument for sending a little metal pellet with great force from one place to another, and it would be the wonderfulness of its working that would strike Carlyle. He would have no special interest in the study of its effects on the making or the movement of wealth and industry. He has no love, for the most part, to any aspect of such inventions but the technical. We must say "for the most part," because in his later books we occasionally get a paragraph or two of strictly economical reasoning (*e.g.*, "Nigger Question," vol. VII., Pop. ed. Essays, pp. 93-4), just as we have occasional relapses into a stern rigour (in dealing with the poor) that would have been thought to sit very ill on the sternest of the old economists or of the modern "organisers" of charitable relief. He couples "philanthropy and the dismal science," and it is not mere philanthropic sentiment that leads him to protest against political economy. He thinks its principles are wrong and that he can, to some extent, show what the right ones are. He shows this conviction even in his earlier books, in Sartor Resartus, 1831, in his Essays from 1827 onwards, and in the French Revolution, 1837; but it was first in "Past and Present," 1843, that he expressed it with all his force. In that book he first states in his own delightfully erratic way the woeful plight of the people of England at the time he wrote. There was wealth in the country, but it seemed to benefit nobody; there were men willing to work, and no employer for them. Manchester weavers had been rising in revolt to bring their condition before the minds of all. "Time was

* Essays, vol. II. (ed. 1866), pp. 296, *seq.* ("Early German Literature"; *cf.* Sart. Resart., bk. I., ch. v.)

† Sart. Resart., bk. I., ch. v., *cf.* bk. II., ch. x.

when the mere handworker needed not to announce his claim to the world by Manchester insurrections." "The world has been rushing on with such fiery animation to get work done and ever more work done, it has had no time to think of dividing the wages; thus merely left them to be scrambled for by the law of the stronger, law of supply and demand, law of *laissez faire,* and other idle laws and un-laws—saying, in its dire haste to get the work done, —that is well enough." ("Past and Present," I., iii.) Yet there is not a horse in England that is not employed and has due wages if he is willing to work. How much is not a man better than a horse? There was a time in England when employment could always be found for men as well as horses. And Carlyle proceeds to tell the story of Brother Sampson in St. Edmund's Monastery, of his becoming Abbot there, and his wise rule over the monks, all to show us how a man's worth was once upon a time recognised without the modern voting paper, and his work was not the less well done because silently, and there was a sense of sacredness in ordinary duties and in common things that has all (Carlyle thinks) completely vanished now. We have made men "free" in the sense of free to go away and not trouble us, "free" in the sense that there is no tie to bind us to them, except the nexus of cash payments. If we employ them, so much the better for them; if we do not, we do them no wrong. The course of events in the leading countries of Europe, and especially in England and France since the Revolution in that country, had been to remove any laws that hindered men from working where they liked, and for whom they liked. The very colliers and salters of Scotland had ceased to be tied to the soil.* But the result was that, as they were bound to nobody, nobody thought himself bound to them. This was the feature of *laissez faire*, or "simple system of natural liberty," on which Carlyle fastened in his "Past and Present." Old ties are gone; no new ties are formed except to be perpetually untied; the result is (he said) "nomadic servitude" of the working classes and social anarchy, along with unlimited competition in the arts of cheating and passing off bad work as good. Nobody (except perhaps Dickens) had brought out so vividly the sombre side of modern industry as Carlyle in this book. The general feeling among educated people had been that the economists who were constantly recommending industrial liberty must be right. Carlyle so far agreed with them

* They remained serfs till the end of last century.

that he thought the Corn Laws must go; the landlords, he said, had not *made* the land of England, and they must not behave like the costermonger, who refused to accept the market rate for his onions. But the emancipation of labour could not be got by the mere repeal of restrictive laws. The "liberty" so secured resulted in absolute uncertainty and insecurity; a man was free to get a job in Glasgow, and to come from London for it, if he liked; but his employers were also free to tell him at the end of it that he must seek work elsewhere, and nobody was bound to see that he got it. The reasonings of the old economists seemed to justify all this; political economy seemed not only to have discovered what was, but to have pronounced it all very good, and left the impression that there could be nothing wrong where there was free competition.

Competition, even to Carlyle, had its virtues. "No man," he says (in the Essay on Scott), "lives by being jostled," he must "elbow himself through the world." In the same spirit of candour, he says there is more virtue in Mammonism than in Dilettantism: "Mammonism has seized some portion of the message of Nature to man, and, seizing that and following it, will seize and appropriate more and more of Nature's message; but Dilettantism has missed it wholly." ("Past and Present," bk. III., ch. iii.) At a later time he becomes more dismal than the dismal science itself, and declares that ideals wont help us far with the vulgar, "who must see indisputable advantage in a thing before they will take to it." (Essays, Pop. ed., vol. VII., p. 232, "Niagara.") The struggle for existence on a large scale abroad is by no means condemned. The native Carib in the West Indies was bound, he says ("Nigger Question," 1849, p. 102), to give place to the white man and the negroes, because "the gods" wish "that something better than swamps and rattlesnakes should be produced there,—nay, more, that, besides pumpkins, which is all that Quashee produces there, spices and valuable products be grown; this much have the gods declared in so making the West Indies."

But at home, in England, he thinks little of the saving virtues of competition, and still less of the economists who praise it. Their science seems to him "dismal," by which he means not dull (for that is a fault of the student, not of his subject), but melancholy in the conclusions to which it seems to lead. For—permanence of conditions, *security of living*—this seems to him the great need in the life of man, if it is to rise above that of the

brute that goeth downwards. And so he looks wistfully, as many in our own generation are doing, to the middle ages and the feudal times, when every man belonged to somebody in the sense that somebody's duty was to care for him. Another reason for his looking back to those days was that in those days people took so much more for granted than they did in his own day; they were so healthy (as he puts it in his essay on "Characteristics") that they did not know they had any stomach. But in our days, these days of self-examination and analysis, nothing is taken on trust, and real peacefulness and faith seem almost impossible.

Yet Carlyle knows that we cannot bring back the days of Abbot Sampson any more than those of Cedric, the Saxon; and, if protestant freedom of inquiry had been forbidden, he would have been the first to rebel; indeed, his wings would have been so effectually clipped that he would never have left his mother earth. His biographer says that late in life he was himself rather alarmed by the use made of his own writings in the direction of free speculation, especially in theology. "*I myself*," he said, "*have given a considerable shove to all that.*"* Yet it is hard to see how complete freedom of thinking, speaking, and writing is reconcileable with any social arrangements like those of the middle ages. And the connection between men's views of the government of the world and their views of the social order they are living under is perhaps closer than we are apt to think it. When men begin to regard society and its customs and laws as altogether confused and wrong, it is not easy for them to regard the entire universe as well governed;—and, on the other hand, when society is well ordered and the people are in the main satisfied with it, the way is prepared for a belief in the good government of the world. To desire, as Carlyle seems sometimes inclined to do, to keep your protestantism but to exclude it from politics, is to wish to separate the inseparable.

Carlyle, on the whole, recognised this; and a curious proof of it is given by the reception of his book *in partibus*. His "Past and Present" was welcomed by the socialists, Marx and Engels, who were then editing from Paris the "German-French Annals," a magazine for Social Democrats, as they would now be called. They hailed the book as the most important published for

* Life, vol. IV. (II. of Later Life), p. 370 *n*.

many years in England on the questions most interesting to them. It even inspired one of them (Engels) to write a book himself on the subject. But they took offence at one feature of it. Carlyle, they said, had not yet succeeded in getting rid of his belief in religion. Few of us will wish to condemn our fellow-countryman for this fault; we should agree that he will rather "be quit for that." The defect of the book lies elsewhere. The author's idea of reconstructing society is not at all so clearly brought out as his criticism of the situation as it then was. He seems to have in his mind the idea of captains of industry and paternal government. Let the *best* lead and govern. Some men are born to rule; most men are born to obey. The relation of servant and master is eternal. The best thing that ordinary men can do is to choose their leader and follow him, to choose their master and be slaves to him; for slavery, he thinks, is the best situation for many men; it is better for the negro to be "hired for life" than it is for the English workman to be hired by the month or the day.*

It is easy to show inconsistencies in Carlyle's views on this head. They appear as soon as we want to know from him how we are to find the best men. He sometimes talks as if they were always immediately recognised and acknowledged—at other times as if they were always neglected or persecuted. But a writer of what is sometimes called "prose poems" must not be tied too tightly to his words. Even if we allow that the best men are recognisable, would this idea that we should simply follow their leadership be the best for social order? Carlyle always has in his mind, when he speaks of a leader, a master; and in our time the two are sternly distinguished. Leaders we must always have; Masters, no longer. But to Carlyle the two are not different. Some men he thinks, are born to be ruled; others (a heaven-chosen few) are born to rule over them. Two of his personal friends are amongst his severest critics here; and, as their criticisms bear more or less directly on the economical aspect of the subject, I make no apology for referring to them.

One is John Stuart Mill, a great contrast to Carlyle in many ways. It is curious that Mill, the Englishman, should have been, above everything, a logician; but he was the son of a Scotchman of the hardest type. Carlyle, a Scotchman of the more spiritual type, scoffs at logic, even when (in some measure) relying on

* "Niagara" (1867), Essays, Pop. ed., vol. VII., p. 204, a quotation from a paragraph written 1863.

it. The difference between the two men was described by Carlyle himself in conversation (See Letters of Caroline Fox, chap. viii., 201):—If Mill and he were in heaven together, Mill would like to know how it all went (how its wheels went round), whereas Carlyle himself would be content to have it as it stood. This was his favourite idea:—Give us the honest men and their drill will come of itself; give us good personal character and the social system will right itself of its own accord. (Life, vol. II., p. 206, &c.) Mill saw, however, that the goodness or badness of the drill might affect the character; and he could not take the machinery of earthly society on trust, whatever he might have done with the heavenly. In his "Political Economy," (published six years after "Past and Present"), Mill introduces a chapter entitled "The Probable Futurity of the Labouring Classes" (bk. IV., ch. vii.), where he states and criticises a theory of "dependence and protection" respecting the condition of labourers, which is substantially that of "Past and Present." The lot of the poor is, according to this theory, he says, to be regulated *for* them and not *by* them; the relation between rich and poor is to be affectionate guidance on the one side, respectful and affectionate obedience on the other. This ideal, Mill adds, has never been realised, and it is quite hopeless now. Men are not capable of being unselfish governors, of the sort described; universal education and independent inquiry would make the governed too critical of their governors. Men must and will learn to depend on themselves; and it is better for their whole character that it should be so. The relation of hireling to hirer is not eternal, but is even now passing into something better, namely, Co-operation. Mill was not criticising Carlyle alone in this passage, or he might have added with truth that Carlyle never understood the significance of workmen's combinations; Co-operation, for example, is to him a mere "drug," or Morrison's Pill.* The union of man with his fellows, their speech to each other, and their joint action, are certainly among his wonders. He marvels at the invisible bond that connects men into societies (see *e.g.*, Characteristics); it is a spiritual tie which suggests to him that other one between the Ruler of the World and His subjects. But a union of men, that does not grow up unconsciously, but is deliberately formed by conscious arrangement and contract, does not im-

* See *e.g.*, "Characteristics," 1831, Essays, vol. III., p. 15, ed. 1866.

press him favourably. It is "mechanical." * Yet it is in Combination that one hope of the English and Scotch and Irish working-men was to lie. Thanks to their unions the dispersed workmen are (as Carlyle desired) in the care of somebody, and it is somebody who knows their wants much better than any master or lord could have done. Carlyle's opinion of trades' societies is somewhat contemptuously expressed in a little fable (of no great merit, it is true), printed at the end of the 1st volume of Essays, and written between 1823 and 1833. The hen cackled loudly for more grain; and, as it was not given to her, she hid her eggs, expecting that surely her masters would yield now; but, instead of that, they drew her neck, and purchased other eggs at sixpence a dozen. No doubt he recognised that the working-classes had at last found a voice; "Balaam's ass" had begun to speak, and "that in a reasonable manner." (Essays, vol. III., p. 178, on "Ebenezer Elliot," 1832.) But, though its claims were reasonable, Carlyle thought nobody but himself knew how they ought to be met.

The dislike of artificial association is connected with a deeper dislike pointed out by Carlyle's other critical friend, Joseph Mazzini. In the papers, which are perhaps the best critical notices of Carlyle ever written—worthy, as they are, alike of the subject and the writer,—Mazzini (Life and Writings, vol. IV., p. 56 and p. 110, date 1843) points out that Carlyle comprehends only the individual, and has no sense of the unity of the race; the progress of humanity by collective labour awakes no enthusiasm in him; he seldom mentions progress except to scoff at it. Like the Comtist, he has a calendar filled with the names of great persons. He has no sympathy with the life of a *people* as distinguished from that of *individuals*. An event like the French Revolution seems to him to be a circular movement;—it is a protest against unrealities, but, having made it, the French nation returns to unrealities;—there is no advance.

Such is Mazzini's criticism, and it points to a defect which shows us how alien to Carlyle any economical study must have been. The economist has to study the acts of individuals only so far as they make up collectively the acts of the whole people; and he who cannot take the collective view is like the husbandman described by Carlyle himself (Essays, ed. 1866, vol. II., p. 173, on "History"): "There are men who labour

* Essays, vol. II., ed. 1866, "Signs of the Times," 1829.

mechanically in a department without eye for the whole, not feeling that there is a whole, and men who inform and ennoble the humblest department with an idea of the whole, and habitually know that only in the whole is the partial to be truly discerned. The simple husbandman can till his field, and sow it with the fit grain, though the deep rocks and central fires are unknown to him; his little crop hangs under and over the firmament of stars. As a husbandman, he is blameless in disregarding the higher wonders, but as a thinker and faithful inquirer into nature he were wrong." It did not occur to Carlyle that what the economists were trying to do was faithfully to inquire into nature in this very fashion. He did not try to do it himself even when his historical subjects almost invited him; the economical and financial causes of the French Revolution are described by picturesque phrases (such as "Astraea Redux, without cash"), but are not lighted up for us by any exposition worthy of them. The economists, including the professors whom he describes "as miserable creatures lost in statistics,"* were for their part toiling to find out what regulated the production and distribution of wealth over the English nation as a whole. Their fault lay not in doing this, but in not doing it thoroughly enough, and in making mistakes in the doing of it. But this only means that, like Carlyle himself, they were but men, and fallible men, whose work was imperfect. It is more philosophical to take up Carlyle's own adaptation of a saying of Aristotle, and say of their theories, as he says of Rousseau's, they are, after all, "processes of nature, who does nothing in vain,"† than to regard them as fuel for fire. With all their mistakes they laid the foundation on which we must build, if we would not repeat their work together with mistakes of our own. One of their mistakes was undoubtedly a tendency to convert their economical conclusions into working principles of practical politics. The fact that they were generally utilitarians ministered to this tendency. By supposing men in industrial operations to follow always the greatest gain they reached certain conclusions which were true on that hypothesis. This was a perfectly lawful way to argue so long as it was not forgotten that the hypothesis was not strictly true, but a large number of other motives besides the greatest gain must be taken into account. For the Statesman, too, the further consideration

* Life, vol. IV. (Later Life, vol. II.), p. 103.

† French Revol., II., vii. (Pop. ed., vol. I., p. 47).

is necessary that the greatest gain of the individual may not be the greatest gain of the whole nation. We can understand how the two things were supposed coincident, by remembering that a consistent follower of Bentham naturally expected to find them so. The simple system of natural liberty is infringed in our time by Factory Acts, Adulteration Acts, and Sanitary Acts, to say nothing of Education Acts. I am not aware that any leading economists follow Mr. Spencer in objecting to each and all of these. We have learned to distinguish politics from political economy. It was Carlyle's "Past and Present" (Mr. Froude thinks) that made the more stringent Factory Acts possible (the half-time system of 1844, in the first instance, and then the Ten Hours Act of 1847). When we remember how many such Acts had preceded them, we may have doubts about this claim; and rigorously taken (if we dared to take Carlyle's words rigorously) Carlyle's tendency is rather towards what is now called An-archism (which is far from meaning anarchy), than towards greater interference by legislation with the conditions of work. His drift is rather:—Reform the individuals and their conditions will reform themselves. If there is one lesson more than another that economists learn by their studies it is that, whether in industry or in other departments of life, the conditions influence the men almost, if not quite, as much as the men influence their conditions.

Carlyle's influence on Political Economy has been essentially of an indirect character. How great or how small an effect his writings have had on economists of the last and of the present generation we need not try to estimate. The discrediting of what is sometimes called the Orthodox political economy, by which is meant the system of Ricardo and MacCulloch, is often said to be the work of Carlyle and Ruskin. But it ought to be remembered that there were many protests raised before any of these authors wrote, and some of these protests were much more logical than "Past or Present" or "Unto this Last," and would have been more convincing if they had only got a hearing. There was always a great body of dissenters alongside of the Established Church of orthodox economics. Not to mention the multitude of Anti-Malthusian books, and of pamphlets on the Currency and the Corn Laws, we have a succession of writings that criticise the orthodox doctrines as a whole. We may classify these dissenters roughly as (1) Articulate, (2) Inarticulate, and (3) Half-Articulate. There were, for example, in the "twenties"

the protests of Hodgskin, Thompson, Combe, Barton, and Read; in the "thirties," of Bray, Gray, Gaskell, and Wade. Hodgskin and Bray, at least, are articulate; they know their opponent's arguments, and seek to meet them by argument. In proportion as the others become more impassioned they become less logical or, if you will, less articulate. This is especially true of the Owenites in the earlier ten years, and the Chartists in the later. Besides these we have the articulate criticisms of Torrens, Chalmers, and Richard Jones, and the vivacious attacks of Cobbett, who is a keen reasoner up to his lights. But Carlyle made what is called the "literary world" and the "educated classes" listen to him, when they had been deaf to all these. Perhaps we may add that he at last got a hearing for these, and prepared the way for his friend John Mill's more logical examination of the old political economy. He did not himself reason with the public; but he forced it to listen to reason, by dint of earnest eloquence in utterance of thoughts which were beyond the reaches of his own soul. He was convinced of the truth of what he preached, and he left other men to look for the proofs of it.

His preaching has left at least one plain effect on political economists. They can never forget any more that their hypotheses are only hypotheses. They can never any more be tempted to identify wealth with all happiness, or to encourage the idea that men are to be measured simply by the money they have made. Phrases like "An Englishman's hell is not getting on" will not soon be forgotten. Plugson of Undershot and Bobus of Houndsditch will be household words for a number of generations yet. Carlyle's own simple way of living was in harmony with his expressed aversion to a materialistic view of prosperity in life, as well as to the dilettantism which "goes gracefully idle in Mayfair." As he finely says in his Reminiscences of his Father, (Rem., vol. II., p. 44):—"The poorer life and the rich one are but the larger or smaller (very little smaller) letters, in which we write the apothegms and golden sayings of life."

The results of this brief glance at Carlyle's relations with Political Economy may be gathered up as follows:—

1st. Carlyle was both preacher and man of letters, and it is mainly as the former that he came into necessary contact with Political Economy.

2nd. In his days the political economists were usually political philosophers; and their political teachings conflicted with his,

which were strongly against Utilitarianism and anything that even seemed to be allied with worldliness.

3rd. He warned his hearers that the economists paid too little attention to the distribution of wealth, and too much to its production; they paid too little attention to the condition of the great body of the people—the working-classes.

4th. More particularly, he thinks that they did not recognise that the effect of free competition is to leave men perfectly isolated from each other, in a state of "nomadic servitude." All permanence and security in the conditions of life are gone.

5th. His remedy is first and foremost that we should all become righteous men, and then that we should put ourselves under our natural leaders, as captains of our industry, and let them order our world for us.

6th. Mill replied that this remedy meant a return to the obsolete system of Patronage: Mazzini, that the suggestion of it went far to show that Carlyle had no real understanding of the progress of humanity, of whole peoples as distinguished from individuals. It was added that Carlyle did not realise the importance of voluntary associations; did not look at industrial movements as a whole, but only in their parts; and, however he deplored the state of the poorer classes, did not recognise the great influence of the external conditions of life on the moral condition of human beings.

But (7th) he has taught political economists to remember that their assumptions are not absolute truths, and that there are other ends in life besides the making of a fortune.

When all is said, Carlyle, it must be admitted, quickens the moral pulse like no other writer of our century. The effect was (and is sometimes still) to send the reader in a direction not intended by the writer. It is said, on good authority (Letters of Car. Fox, p. 139), that a man once called to thank Carlyle for having converted him from Quakerism to Jeremy Bentham, and from Jeremy Bentham to the faith of the Roman Catholic Church! This was, no doubt, an exceptional case. But no man has ever been possessed by Carlyle without becoming a better man for it, or at least learning more of what righteousness is. As to his power as a writer, that power is so great that it is difficult (especially for his countrymen) to criticise him without first closing the volumes.

For students, of any and every branch of science or literature, he has a special virtue. Carlyle has done for literature and history what Ruskin has done for the plastic arts. He has brought home the obligation of all workers in those fields to make such work as falls to them as perfect as possible, "not with eye service as men pleasers, but in singleness of heart, fearing God." And, as political economists, we may learn from him a lesson which he hardly meant to teach:—that the study "of the nature and causes of the wealth of nations" must be recognised by us to have as much sacredness in it as the study of any of the other revelations of the divine "invisible hand" working in the world of plants, or of animals, or of the stars of heaven. The phenomena are not less wonderful because they are so familiar and so much (we may think) of our own making. Economists, more perhaps than other men, need to turn to literature and history to correct their tendency to rely on abstractions, and look merely to what is of the earth earthy. From this literary man and historian, in particular, they will receive the doctrine, reproof, and correction that seem at present most needed by them.

Our younger students hardly need this exhortation. At a certain stage in the career of a youth at our Scottish universities, and even in the South, Carlyle may be said to overmaster the mind as a fever the body; and, as some fevers are said to kill the seeds of other diseases, and actually leave the patient stronger than he was before them, so we may say there are many mental evils that find it hard to grow up in the mind of any one who has ever been possessed by Carlyle. This applies to us all, even when our later pursuits, in business or in study, have cured us of all idolatry in literature, by showing us how wide is the range of worshipful objects.

IX.—*On Language.* By F. MAX MÜLLER, M.A., Professor of Comparative Philology in the University of Oxford, and Honorary Member of the Philosophical Society of Glasgow.

[Read before the Society, 21st January, 1891.]

IT seems impossible to many people to look upon language as anything but an instrument of thought. In one sense this is perfectly true. We think by means of words, just as we see by means of eyes, and hear by means of ears, and walk by means of legs. But could we walk without our legs, or see without our eyes? We can walk with artificial legs, no doubt, and so we can think and speak in a foreign language, and in every kind of artificial sign-language. But as artificial legs presuppose natural legs, foreign and artificial languages presuppose our own natural language.

When we speak of instruments we mean generally such things as knives with which we cut, or pens with which we write. They are instruments which are useful, but they are not indispensable, and can be replaced by other instruments. This does not, however, apply to eyes, ears, or language, and in order to mark that distinction the former are generally called instruments, the latter organs. Now, if we call language the organ of thought, we, no doubt, admit that we can distinguish between the *organon*, that which works, and the *ergon*, *i.e.*, the work which it performs. But it does by no means follow that therefore the *ergon* could ever exist without the *organon*. We can easily distinguish between the act of spoken thought and the organ of spoken thought; but it does by no means follow that therefore the act of spoken thought could ever exist without the organ of spoken thought.

It may seem unfair in this argument to call thought, spoken thought. It looks like begging the whole question. But it really is not so. By calling thought, spoken thought, we only supply

a deficiency of our modern languages. If we were Greeks, we should use the simple word *logos*, and, instead of begging the question, we should show that our proposition is really self-evident, or, it may be, even tautological, namely, that logos is impossible without *logos*.

Here we can see at once how intimately thought is connected with language, how it is dependent on it, or more correctly, how inseparable the two really are. If, like the Greeks, we had a word such as *logos*, we should probably never have doubted that what we call speech and thought are but two sides of the same thing. And the same lesson is taught us again and again, if only we are inclined to listen to it.

Suppose we had no such word as *matter*, would not our whole system of thought be different? Matter is not an object perceived by our senses. We may even go further and say that matter by itself never exists. This or that matter exists; chemical substances, say gold or silver, oxygen or hydrogen, exist; but matter, which some philosophers look upon as the most certain and concrete of all things, is simply an abstraction, something that may be predicated of many things, but that is never found by itself *in rerum naturâ*. Some people define matter as what is ponderable and impenetrable; but here again, nothing exists that is simply ponderable or impenetrable. It is always something else, it is iron, wood, stone, vapour, gas, but never matter *pur et simple*.

It is clear, therefore, that matter is made by us, and that without some such word as matter we could never have the faintest idea or concept of matter. For how should we call it? On the other hand, it is equally clear that we could not have the word matter without the concept of matter. For what would be the use of it? Now, what follows from this apparent dilemma? If the concept cannot be prior to the name, and the name cannot be prior to the concept, they must needs be simultaneous, or, more correctly, they must be the same thing under two aspects.

From an historical point of view, that is, if we consider the genesis of words and concepts, not in modern times, but during that period when words and concepts were framed for the first time, we are bound to admit that the word is really the *prius*. That period may be ever so far distant, but it was nevertheless a very real and truly historical period.

How, for instance, did man arrive at such a word as matter? The word tells its own story. It came to us from French, it came into French from Latin. In Latin *materies* or *materia* still means wood and timber, though it has also assumed the meaning of matter, like the Greek ὕλη, which means both wood and matter. The process by which *materies* came to mean matter is clear. If *materies* meant originally the wood out of which a hut, a table, a chair, or a stick was made, it was naturally applied to other substances also, such as stone, bricks, or metal, when used in the making of huts, tables, chairs, or sticks. In the same way we speak of a pen, *i.e.*, a quill, though we mean a steel pen.

When the original special meaning of wood thus disappeared, there remained only the meaning of building material, and, at last, of matter and substance. We say now, What is the matter? What does it matter? but we little think of the solid beams out of which such expressions were hewn and fashioned.

In this sense, therefore, we may say that historically the word *materies* came first, meaning a beam, and that gradually it shed its various attributes, one after the other, till there remained nothing but its trunk, and that is what we now mean by matter.

Here, therefore, we see the process of generalisation, which is very important, particularly in the later periods of language and thought.

But it is the greatest mistake to suppose that language, such as we know it—what we might call historical language—always begins with the particular, and then proceeds to the general. Adam Smith was one of the ablest defenders of the theory that the *Primum Cognitum*, and the *Primum Appellatum*, must have been the particular. But all the facts of language are dead against this theory. And yet that theory has once more been put forward by a philosopher who prides himself on nothing so much as that his philosophy rests throughout on positive facts. I do not blame a philosopher who is ignorant of the results obtained by the Science of Language, so long as he abstains from touching on the subject. But constantly to appeal to language, and yet to ignore what has been achieved by comparative philologists, is unpardonable. No one is a greater sinner in that respect than Mr. Herbert Spencer.

When speaking of the process by which the abstract idea of colour was formed, he says:*—"The idea of each colour had

* "Data of Ethics," p. 127.

originally entire concreteness given to it by an object possessing the colour, as some of the unmodified names, such as orange and violet, show us. The dissociation of each colour from the object specially associated with it in thought at the outset, went on as fast as the colour came to be associated in thought with objects unlike the first, and unlike one another. The idea of orange was conceived in the abstract more fully in proportion as the various orange coloured objects remembered cancelled one another's diverse attributes, and left outstanding their common attribute. So it is if we ascend a stage, and note how there arises the abstract idea of colour apart from particular colours."

Now this is all untrue. Such names as orange and violet are some of the latest names of colour. They presuppose such late, nay exotic concepts, as *orange* and *violet.* The question why an orange was called an orange, and a violet a violet, remains unasked and unanswered. In the old names for *black, white, red, green,* and *blue,* there is no trace of ink, or snow, or blood, or sea, or sky. They are all derived, so far as we can analyse them at all, from roots meaning to shine, to grow, to beat black and blue, and not from oranges, roses, and violets.

Again, what can be the meaning of such a sentence as * :— "Words referring to quantity furnish cases of more marked dissociation of abstract from concrete. Grouping various things as small in comparison either with those of their kind or with those of other kinds; and similarly grouping some objects as comparatively great, we get the opposite abstract notions of smallness and greatness." Does Mr. H. Spencer really believe that we can call things small and great, that our language can possess two adjectives expressive of these qualities, and that yet at the same time we are without an abstract notion of smallness and greatness?

Mr. H. Spencer constantly calls on the facts of language to confirm his views, but his facts are hardly ever correct. For instance, after having explained† that, according to his ideas, greater coherence among its component motions broadly distinguishes the conduct we call moral from the conduct we call immoral, he appeals to the word *dissolute,* when meaning immoral, as proving this theory. But *dissolutus* in Latin meant originally no more than negligent, remiss. *Dissolutio* meant langour, weakness, effeminacy, and then only licentiousness and immorality.

* Loc. cit., p. 125. † Loc. cit., p. 66.

Language, therefore, in no way confirms Mr. H. Spencer's speculations, still less does experience, for no man is often so coherent in his acts, so calculating, so self-restrained as the confirmed criminal; no one is often so careless, so little shrewd, so easily duped as the thoroughly moral, and therefore trustful and confiding man.

But to return to the history of the word for matter. The process by which *materies*, wood, came to mean matter, is intelligible enough, whether we call it generalisation or abstraction. But how came materies to mean wood? That is the question which has to be solved, and in solving it, we shall find that while in the second period of thought-language, the progress is from the particular to the general, the progress in the first period is the reverse, namely, from the general to the particular.

In the case of *materies* this is very clear. No one can doubt that in *materies* the radical element is *mâ*, the derivatives *ter* and *ies*. The radical element *mâ* or *mă* is found in Sk. *mâ-tram*, measure, *mâ-nam*, measuring, *mâ-na-s*, a building; in Greek, *μέ-τρον*, measure, in Latin, *mê-tare*, to measure. We can hardly doubt that the oldest Aryan name for mother also, namely, *mâtar*, Greek *μήτηρ*, Lat. *mater*, English *mother* is derived from that root, though it is doubtful in what sense. It may have meant originally no more than maker or fashioner, and it is important to observe that in the Veda the word *mâtar* does actually occur as a masculine, and means maker, and governs an accusative. But it may also have meant arranger, controller, and mistress of all household affairs. Whatever its original intention was, *mater* soon became a mere name. Its etymological key-note was no longer audible, and *mater* simply meant mother and all that was implied in that name, when used by children and others.

If we compare all the words which contain this *mâ* as their common element, we can see that it meant originally to put two or more things together. This led to two applications. What we call measuring is really putting two things together, one by the side of another, to see how far they agree and how far they differ. Thus *mâ* took the special meaning of measuring, in such words as Gr. *μέτρον*, and Sk. *mâtram*. But to put together could also be used in the sense of joining, carpentering, building, and making, and this meaning we find in such words as Sk. *mânas*, a building; *mâti*, he measures, he makes; and likewise in *materies*, what has been fashioned, what can be used for building a hut, timber, wood,

building material, then, any kind of material, and at last, matter, substance in its most general acceptation.

You can see here very clearly the twofold process in the formation of words, first, from the general to the particular—from measuring to wood,— and then from the particular to the general —from timber to matter.

If you ask, what is this syllable *ma* which has the general meaning of measuring and making, I can only answer, we know and we do not know. We know, as a fact, that it is the common element in a number of words, which are differentiated by a number of derivative elements, called suffixes, prefixes, and infixes, but which can all be shown to share in common the general meaning of making and measuring. These common elements have been called roots. The question whether these roots ever existed by themselves, and whether any language could ever have consisted of such roots only, is a foolish question. For as soon as a root occurs in a sentence, it is either a subject or a predicate, a noun or a verb, and it has ceased to be a mere root. But on the other hand, it is quite true that in certain languages, as, for instance, in Chinese, there is no *formal* difference between a root and a word—there are no suffixes or prefixes. But the strict rules of the collocation of words in every sentence make it quite clear in Chinese whether a word is to be taken as a substantive, a verb, an adjective, an adverb, and all the rest.

By the same process by which we have reduced a number of words to the root *ma*, the whole dictionary of Sanskrit, and of English also, in fact of all the Aryan and likewise of the Semitic languages, has been reduced to a small number of roots. Given that small number of roots, we undertake to account for the whole wealth of words in any language, simply by means of derivation with suffixes and prefixes, and by means of composition.

In all this we are dealing with facts, facts which are as well ascertained as any facts in physical science.

Making allowance for a small margin of words, which have as yet resisted all attempts at etymological analysis, we can state that the vast majority of words in Sanskrit has been reduced to about 800 roots. In the progress of language whole families of words derived from some of these roots become extinct, while others continue prolific, and take their place. The consequence is, that the number of roots in English has dwindled down to 461, while the sum-total of words has risen to about 250,000.

Every one of these roots has a general or conceptual meaning, such as striking, pushing, rubbing, cutting, bearing. binding, measuring, building, moving, going, falling, and all the rest.

It often happens, however, that two or more roots have the same, or nearly the same, meaning, and this explains why, when we count the fundamental concepts expressed by our 800 roots in Sanskrit, we find that they amount to no more than 121.

I say, again, that in all this we are dealing with well ascertained facts.

The next step, however, leads us into the domain of theory. If we are asked how these roots came into existence, we can either decline to answer the question as outside the limits of our science. A chemist would probably do the same, if he were asked how the chemical elements came into existence. In fact, the students of the Science of Language have always taken their stand here, and have treated roots as ultimate facts.

I ought to mention, however, two theories, which, though they have long been surrendered by students of the Science of Language, still enjoy a certain popularity, and commend themselves to many people by their simplicity and plausibility.

The first consists in ascribing the roots of all languages to a direct communication from God. It is impossible to refute such an opinion; all we can say is that such a communication, if we try to realise it in imagination, would imply so crude an anthropomorphism that one naturally shrinks from entering into details.

The second consists in looking upon roots as imitations of the sounds of nature, or as interjections. Here all we can say is, that the experiment has been tried again and again, and has failed. Every language contains a number of such words which are imitations of the sounds of nature, or interjections. No one can doubt of the origin of *bow-wow*, a dog, or of *pooh-poohing*, in the sense of rejecting. But the great stock of words cannot be accounted for by this easy process, and no serious scholar would think of resuscitating what many years ago I described as the Bow-wow and Pooh-pooh theories.

But while the student of language seems to me to have a perfect right to treat the roots of language as ultimate facts, it is difficult for the philosopher not to look beyond. He cannot hope to do more than to suggest a hypothesis, but if his hypothesis accounts for the few facts he has to deal with, such a hypothesis is legitimate, though, no doubt, it is very far from being an established truth.

The hypothesis which I suggested on the origin of roots was suggested to me by Professor Noiré's hypothesis as to the origin of concepts. My late friend, Professor Noiré, was one of those who discovered difficulties where no one else saw them. While most philosophers were satisfied with the fact that man possessed the power of forming, not only percepts, but concepts also, though no trace of conceptual thought was found in animals, Noiré subjected this power of forming concepts to a most minute psychological analysis, and thus was brought face to face with the question, what was, from a psychogenetic point of view, the real impulse to the formation of conceptual thought. Questions like this, which to most people seem perfectly superfluous, often mark the real progress in the history of philosophy. Logicians see no difficulty in explaining how, either by addition or subtraction, positively or negatively, concepts are formed out of percepts. White, they say, is either what snow, milk, and marble share in common, or what remains, if we drop from snow, milk, and marble all but their colour. The psychologist, however, who looks upon the human mind as the result of an evolution, whether in the individual or in the race, asks, not *how*, but *why* such concepts should have been formed. Now, Professor Noiré showed, as I thought, with great sagacity that the first inevitable concepts arose from man's consciousness of his own repeated acts; and that nowhere in nature could we find a similar primitive and irresistible impulse to conceptual thought. If the beginning had once been made, there was no longer any difficulty in accounting for the further development of conceptual thought in all directions.

I call this no more than a hypothesis, or, if you like, a guess; and I do not see how, in the regions in which we find ourselves, we can expect anything more than a hypothesis. But when one hypothesis, like that of Noiré's, harmonises with another hypothesis that was formed quite independently, we cannot help seeing that the two lend each other powerful mutual support.

Let us remember, then, that a most careful psychological analysis had led Noiré to the conclusion that the germs of all conceptual thought were to be found in the consciousness of our own repeated acts. And let us place by the side of this the well-ascertained fact that the germs of all conceptual language, what we call the roots, express, with few exceptions, the repeated acts of men. Is not the conclusion almost inevitable that these two processes were in reality but two sides of one and the same

process in the evolution of human thought and human language? Professor Noiré did not know of the linguistic fact when he arrived at his psychological conclusions. I did not know of his psychological conclusions when I arrived at my linguistic facts. But when I saw that by different roads we had both arrived at exactly the same point, I thought that this could not be a mere accident.

There remained, however, one more question to be answered, and that question again could be answered hypothetically only. How can we account for the sounds of the roots, which we have recognised as the germs of conceptual thought and conceptual language? Why should, for instance, the concept of rubbing be expressed by MAR, and that of tearing by DAR? Here again Noiré and others before him have pointed to the well-known fact that men, when engaged in common acts, find a relief in emitting their breath in more or less musical modulation. It has therefore been supposed that our roots are the remnants of sounds which accompanied these acts, and which, being used, not by one man only, but by men acting in common, were therefore intelligible to the whole community.

No one would dream of representing this theory of the origin of our conceptual roots as a well-ascertained historical fact. It is, and can only be, a hypothesis. But, as such, it fulfils all the requirements of a working hypothesis. It explains all that has to be explained, and it does not run counter to any facts or any well-established theories. It explains the sounds of our roots, not as mere interjections, which would be the signs of momentary feelings, and not, what we want, the signs of our consciousness of a number of repeated acts as our actions. Our roots are, if we may venture to say so, conceptual, not interjectional, sounds. They are, in fact, exactly what, according to Noiré's philosophical system, the primary elements of language ought to be.

I do not say that this theory is the only possible theory of the origin of roots, and therefore of language. Let a better theory be started, and I shall be delighted to accept it. But don't let us try to revive exploded theories, unless there are new facts to support them. I can only give you my own experience. For many years I was satisfied to look upon roots as ultimate facts. But when Professor Noiré showed that the fundamental concepts of our thought must be concepts expressive of our own acts, and when thereupon I went carefully through the list of our Aryan roots,

and found that, with few exceptions, every one of them, as a matter of fact, expressed the ordinary acts of men in a simple state of civilisation, I was driven to the conclusion that the primitive roots of Aryan speech owe their origin to the sounds which naturally accompany many acts performed in common by members of a family, a clan, or a village. This would vindicate once more the conviction which I have always held, that language was from the beginning conceptual, and confirm the well-known statement of Locke, that "the having of general ideas is that which puts a perfect distinction between man and brutes, and is an excellency which the faculties of brutes do by no means attain to."

Allow me, in conclusion, to say a few words on what I can hardly call a criticism, but rather a misrepresentation, or, I ought perhaps to say, a complete misapprehension of this theory of the origin of roots, which you may have seen in a book lately published by Professor Romanes, "Mental Evolution in Man," as a continuation of an earlier work of his, called "Mental Evolution in Animals." My learned friend, Professor Romanes, labours very hard to show that there is an unbroken mental evolution from the lowest animal to the highest man. But he sees very clearly, and confesses very honestly, that the chief difficulty in this evolution is language, and all that language implies. He tries very hard to remove that old barrier between beast and man. For that purpose he devotes a whole chapter, the thirteenth, to a consideration of the roots of language, and yet he says at the end of the chapter, "I wish, in conclusion, to make it clear that the matter—that is the question whether roots are imitations of sound and interjections—is not one which seriously affects the theory of evolution."

If it were so, why should Professor Romanes have devoted a whole chapter to it? But it is not my intention to argue this question with Professor Romanes, but rather to show how difficult it is for any one, not acquainted with the Science of Language, even to apprehend the problems that have to be solved. Professor Romanes is, I believe, a most eminent biologist, and the mantle of Darwin is said to have fallen on his shoulders. Far be it from me to venture to criticise his biological facts. But we see in his case how dangerous it is for a man who can claim to speak with authority on his own special subject, to venture to speak authoritatively on subjects not his own.

Professor Romanes has, no doubt, read several books on philology and philosophy, but he is not sufficiently master of his subject to have any right to speak of men like Noiré, Huxley, Herbert Spencer, to say nothing of Hobbes, with an air of superiority. That, of course, is entirely out of place. When he points out differences of opinion between philologists, he does not even understand how they have arisen, and he ought to know better than anybody else that mere difference of opinion between two competent scholars does not prove that both are wrong, and can never be used to throw discredit on the whole science.

But as I observed just now, I am not going to argue with Professor Romanes, because, as he says himself (p. 276), if I were right, his whole theory would collapse. I hope this is not the case, but I feel sure that if it were so, Professor Romanes, in the interest of truth, would only rejoice at it. Anyhow, why introduce so much of the *meum* and *tuum* into these discussions? If it could be proved, for instance, that the Âryas came from Europe, then, no doubt, the other theory that they came from Asia would collapse. But among serious students, any such collapse would be greeted with gratitude, and would be looked upon simply as a step in advance. We are all fellow-workers, we all care for one thing only, the discovery of truth. It is in this spirit, and without a thought of any collapse, that I venture to point out a number of clear mistakes which occur whenever Professor Romanes touches linguistic questions, and which fully account for his not perceiving the true character of the evidence placed before us by the Science of Language.

On p. 267, he says that I profess, as a result of more recent researches, to have reduced the number of Sanskrit roots to 121. I wish I had. But the number of roots in Sanskrit stands as yet at about 800; the number of 121 of which he speaks is the number of concepts, expressed by these roots, many of them conveying the same, or nearly the same, idea. A root is one thing, a concept quite another. To confuse the two is like confusing thought and expression.

I thought I had made it quite clear that these 121 concepts conveyed by about 800 roots, are simply and solely the residue of a careful analysis of Sanskrit, and of Sanskrit only. I took particular care to make this clear. "They constitute the stock-in-trade," I said, "with which every thought that has ever passed through the mind of India, so far as it is known to us in its

literature, has been expressed." What can be clearer? Still Professor Romanes thinks it necessary to remark that "these concepts do not represent the ideation of primitive man." I never said they did. I never pretended to be acquainted with the ideation of primitive man. All I maintained was that, making allowance for obscure words, every thought, that of the lowest savage as well as that of the most minute philosopher, can be expressed with these 800 roots, and traced back to these 121 concepts. I even hinted that the number of these concepts might be considerably reduced. The question is not whether certain acts, such as *to yawn*, *to spew*, *to vomit*, *to sweat*, were of vital importance to the needs of a primitive community, but whether, as a matter of fact, they were known and therefore named in the early vocabulary of India. If, on the other hand, some of these concepts, such as *to cook*, *to roast*, *to measure*, *to dig*, *to plat*, *to milk*, betoken an advanced condition of life, all we can say is, that they would probably not occur in the dictionary of the primeval savage, wherever such a being can be found, and that they do not profess to be the first utterances of the *Homo alalus*, whoever that may be.

Immediately after this, Professor Romanes dwells on what he calls the interesting feature of all roots being verbs. This is simply a contradiction in terms. In giving the meaning of roots, scholars generally employ the infinitive or the participle, to go or going, but they have stated again and again that a root ceases to be a root as soon as it is used in a sentence, either as a subject or as a predicate, either as a noun or a verb. All his arguments, therefore, that archaic words expressive of actions would have stood a better chance of surviving as roots than those which may have been expressive of objects, are simply out of place. The question whether verbs come first or nouns may be argued *ad infinitum*, quite as much as the question whether the egg came first or the chicken. Every sentence requires a subject as well as a predicate. If Professor Romanes approves of my saying that roots stood for any part of speech, just as the monosyllabic expressions of children do, I can only say that if I ever said so, I expressed myself incorrectly. A root never stands for any part of speech, because, as soon as it is a part of speech, it is no longer a root.

After that, Professor Romanes returns once more to his statement that the roots of Aryan speech are not the aboriginal elements of language as first spoken by man. Why deny what has never been asserted? I know nothing of the language as first spoken

by man. I say with Steinthal, "Who was present when the first sound of language burst forth from the breast of the first man, as yet dumb?" All that we, the students of language, undertake to do is to take language as we find it, to analyse it, and to reduce it to its simplest component elements. What we cannot analyse, we leave alone. The utmost we venture to do is to suggest a hypothesis as to the possible origin of these elements. Of the *homo alalus*, the speechless progenitor of *homo sapiens*, with whom Professor Romanes seems on very intimate terms, students of human speech naturally know nothing. Professor Romanes assures us (p. 211) that the reducing of language to a certain small number of roots, and the fact that all the roots of language are expressive of general and generic ideas, yield no support whatever to the doctrine either, that these roots were themselves the aboriginal elements of language, or, *a fortiori*, that the aboriginal elements of language were expressive of general ideas. He evidently does not see that we are speaking of two quite different things. I am speaking of the facts of language, he is speaking of the postulates of a biological theory, which may be right or wrong, but which certainly derives no support whatever from the Science of Language. If, like Professor Romanes, we begin with the "immense presumption that there has been no interruption in the developmental process in the course of psychological history," the protest of language counts for nothing, the very fact that no animal has ever formed a language is put aside simply as an unfortunate accident. But to students to whom facts are facts, immense presumptions count for nothing: on the contrary, they are looked upon as the most dangerous merchandise, and most likely to lead to shipwreck and ruin.

Instead of closing with these facts, Professor Romanes tries to show that those who try to explain them are not always consistent. That may be so, and I should be sorry, indeed, if my latest views were not more advanced and more correct than those which I expressed forty years ago. But very often where Professor Romanes sees inconsistency, there is none at all. Speaking of roots in my "Science of Thought," I said, "although during the time when the growth of language becomes historical, and most accessible therefore to our observation, the tendency certainly is from the general to the special, I cannot resist the conviction that before that time there was a prehistoric period during which language followed an opposite direction. During that period roots, beginning

with special meanings (though, of course, always general in character), became more and more generalised, and it was only after reaching that stage that they branched off again into special channels."

The observation which I recorded in these words was sipmly this, that a root, meaning originally to yawn, may in time assume the meaning of opening, while, during a later period, a root, meaning to open, may come to be used in the more special sense of yawning. Facts are there to prove this. But whether a root expresses the act of yawning or opening, it remains general and conceptual in either case, though the intention of the concept may be smaller or larger. Where Professor Romanes sees inconsistency, he only shows that he has not apprehended the drift of my remarks.

When all the facts of real language are against him, Professor Romanes betakes himself to baby-language. Here he is safe, and he knows quite well, why I refuse to argue with him or any other philosopher either in the nursery or in the menagery, either about mamma and papa, or about "poor Polly." But if all he wants is to prove the possibility of onomatopoieia, he could have found much ampler evidence in my own laboratory, only with this restriction, that, after we have analysed these onomatopœic words, which in some languages are far more numerous than even Professor Romanes seems to be aware of, we are only on the threshold of the real problem, namely, how to deal with real language, that is, with those conceptual words which can*not* be traced back to natural sounds or interjections.

Professor Romanes appeals to philology in support of his theory, and, to use a favourite phrase of his own, to philology let him go. It was long considered an irrefragable proof in support of the onomatopœic theory that *thunder* was called *thunder*. People imagined they heard the rumbling noise of the clouds echoed in the sound of thunder. However, the word was taken to pieces by comparative philologists. *Thunder* was found out to be closely connected with the Latin *tonitru*, and Sanskrit *tanyatu*, and there could be no doubt that these words were all derived from the root TAN, to stretch, from which Gr. τόνος, meaning stretching, tension, and tone. *Thunder*, therefore, was clearly shown to owe its origin to this root *tan*, in which there is very little trace of distant rumble. But what does Professor Romanes do? He appeals in his distress to Archdeacon Farrar, who is

reported to have said that the word *thunder*, even if not originally onomatopœic, became so from a feeling of the need that it should be. Now, this fairly takes away one's breath, and I cannot believe that Professor Romanes could have used this argument seriously. He begins by maintaining that words are formed by imitation of natural sounds. He quotes *thunder* as a case in point. He is told by comparative philologists that *thunder* is derived from a root TAN, to stretch. He does not attempt to deny this, but he appeals to Archdeacon Farrar, who says that the word became afterwards onomatopœic, from a feeling of the need that it should be so. If that is not shirking the question, I do not know what is. Suppose it were true that thunder had been supposed to be an imitation of a rumbling noise by those who, like Professor Romanes, are convinced that all words must be more or less onomatopœic,—what in all the world has that to do with the real origin of the word? We want to know how the word thunder came to be, and we are told, if it was not onomatopœic, it ought to have been so, nay, that by certain ignorant people it was supposed to be so. This goes beyond the limits of what is allowed in any serious discussion.

But Professor Romanes attempts a still greater triumph in forensic adroitness, when he suddenly turns round and declares himself almost convinced by the theory proposed by Noiré and myself, though, at the same time, placing it on a level with the Bow-wow and Pooh-pooh theories. Now the fact is, and Professor Romanes knows it perfectly well, that both Professor Noiré and myself have been most anxious to show the fundamental difference between these two exploded theories and our own. He knows that the theory which I, for clearness sake, was willing to call the *Yeo-he-ho* theory, is the very opposite of the *Synergastic* theory. Those who appeal to words like *thunder* as derived direct from the rumbling sound in the clouds, without any conceptual root standing between our conceptual and these unconceptual noises, hold the Bow-wow theory. Those who hold that *fiend* is derived direct from the interjection *fie*, without any conceptual standing between the unconceptual *fie* and the conceptual word *fiend*, hold the Pooh-pooh theory. Those who would derive *to heave* or *to hoist* from sounds like *Yeo-he-ho*, would hold what may be called the Yeo-he-ho theory. I have never denied that there are some words in every language which may be so explained.

But what similarity is there between these theories and our own? We begin with the fact that the great bulk of a language consists of words derived, according to the strictest rules, not from cries like *cuckoo* or *fie*, but from articulate roots. No one denies this. We follow this up with a second fact, that nearly all these roots express acts of men. No one denies that. We then propound a hypothesis that possibly the phonetic elements of these roots may be the remnants of utterances, such as even now sailors make when rowing, soldiers when marching, masons in pulling and lifting, and that as expressing originally the consciousness of such repeated acts, performed in common, these roots would fulfil what is wanted, they would express conceptual thought, such as beating, cutting, rubbing, binding, and all the other 121 concepts from which, as a matter of fact, all the words that fill our dictionaries, have been derived. If Professor Romanes cannot see the difference between a man, or, for all that, between a mocking-bird, saying *Cuckoo*, and a whole community fixing on the sound of TAN, as differentiated by various suffixes and prefixes, and expressing the concept of stretching in such words as *tonos*, *tone*; *tonitru*, *thunder*; *tanu*, *tenuis*, *thin*—he should keep aloof from the Science of Language.

His observations on the language of children, or on what I call nursery philology, are no doubt very interesting, and may be useful for other purposes. But what have they to do with the problem of the origin of language? The two problems, how a child learns to speak English, and how language was elaborated for the first time, are as remote from each other as the two poles. The one is perfectly clear, though it may vary in different children. No child *makes* its language; it simply accepts what has been made. What *we* are concerned with is—how each word was originally made; how the first impulse to speech was given; what were the rough materials out of which words were shaped; how words assumed different meanings by becoming specialised or generalised, or by being used metaphorically; and how, in the end, some words became purely formal, or served as the grammatical articulations of human speech. What has all that to do with a child learning to say *Bread* or *Milk*, or with a parrot learning to say *Poor Polly?* We might as well try to study the geological stratification of the earth from watching the layers of a wedding-cake. I know quite well that every philosopher, when he becomes a father, thinks that he may discover the origin of

language in his nursery. The books which owe their origin to these paternal experiments are endless. But they have thrown hardly one ray of pure light on the dark problem of the origin and evolution of human speech. That problem, if it can be solved at all, can only be solved by a careful analysis of language, such as it exists in the immense varieties of spoken languages all over the globe. This is the work which the Science of Language has carried out for nearly a century, and which will occupy the minds of many students and philosophers for centuries to come.

X.—*Iceland: Some Sociological and other Notes.*

By Professor JAMES MAVOR.

[Read before the Society, 4th February, 1891.]

ICELAND, like Africa, lies on "the threshold of history." Although it is no farther from John o' Groat's House than John o' Groat's House is from London, it is so far removed from those countries whose temperate climate permits the growth of large populations with highly organised industry and commerce, that it is beyond the range of their rush of life and their continuous technical and social change. To the geologist and to the member of the Alpine Club, its unexplored deserts of sand and snow, and its unconquered glaciers and volcanic peaks; to the sportsman its salmon rivers; and to the tourist its invigorating fresh air, are attractions powerful enough to draw them so far into northern seas. The demon amateur photographer too has organised himself into expeditions, has planted his camera in the very crater of Hekla, and has even, it is said, with a Kodak detected Geyser in the act of erupting.

Apart from its unique physical characters, Iceland has a special interest, for it is the home of the descendants, by almost unmixed generation, of the race to which the whole of the north European peoples are more or less nearly related, whose blood, in fact, runs in our own veins, and in those especially of the Norwegians and of the peasants and fishermen of Normandy. Our own stock is so largely that of the Norse branch of the Scandinavian, that the Icelanders, as the children of the Vikings, are more nearly our cousins than are any other people. In the economical and social conditions of Iceland, therefore, we may find at least a partial picture of our own country as it was under Norse influence. It is only a partial picture, because at no period of our history have we been otherwise than affected by diverse racial influences, and the physical character of our country has rendered possible a development to which Iceland is, and probably must remain, a stranger. Yet the needs and habits of the people are so far like ours that their means of meeting them are the same as ours have been at a certain phase of our national development. Thus, instead

of attempting to imagine vaguely what the character and condition of the English and Scotch people would have been now, had they missed the infection of the commercial spirit, we need only look at Iceland. It offers in this sense an opportunity for an investigation in comparative sociology, which at present can only be suggested and outlined. If the Romans had not inoculated us with the diseases of running about and of the planning of roads and bridges to run about upon; if Watt had simply let the kettle boil instead of puzzling his brains about the steam issuing from it; if Stephenson, Siemens, Wheatstone, Nasmyth, *et id genus omne*, had devoted themselves to pure science instead of to the applications of it: we might have had no roads nor bridges, no steam engine as we know it, no locomotive, no steel, no telegraph, no steam hammer; and we might have had no factory system, nor docks, nor strikes, nor trade unions, nor joint-stock companies, nor syndicates, nor millionaires, nor paupers, nor criminals, nor jails, nor workhouses, nor standing army, nor navy, nor policemen: if we had escaped being swallowed up by more aggressive nations, we might have been a high-toned, self-controlled, and cultivated people; there would not have been many of us, but we should have been very fine. Some of these things do not exist, and the rest are rare in Iceland; and yet, somehow, the Icelanders are not wholly barbarous. On the contrary, they are supremely civilised. They are among the most expert horsemen, the best caligraphists, the best printers, the best archæologists, the best makers of coffee in the world, and as if these accomplishments were not enough, they are musical, learned, and courteous, and their moral tone is distinctly higher than that of any people in Europe.

THE PEOPLE.

"Silver and gold the gods have denied them, whether in mercy or in wrath I am unable to determine." They are blue-eyed and yellow-haired (inclining to red)—not universally, for many are dark; they intermarry but little with other nations; they have an intense love of freedom; they are effusively hospitable; they have a strong feeling of clannishness; they like to loiter, though at times they are extremely energetic; they dislike getting up early in the morning; they are, or rather have been (for there is a great recent change in this respect),* fond

* During the past few years a vigorous temperance movement has been carried on, especially at Reykjavík.

of drinking; they are good horsemen. These items are all taken from the "Germania" of Tacitus, and they apply exactly to the Icelanders of to-day. The Icelander has a certain very charming native tranquillity of manner. He is never in a hurry or put about. He is always modest, dignified, and gracious. He is simple—indeed, naïve, as a rule; but behind his simplicity there is an invincible astuteness which never leaves him in the lurch in a bargain. He is very honest and very just, he may willingly forego a claim; but he is not to be taken advantage of. Leopardi's account of the Icelander fleeing from Nature* is at least partly true, although the particular truth of it, so far as the Icelander is concerned, was not of moment for Leopardi's purpose. The Icelander is indeed a creature pursued by frost and fire. His short summer is a time of incessant toil, and his long winter a period of shivering inaction.† The gloom of the eight months of winter, the perpetual desolation of a vast part of the whole land, the frequent storms that make the life of a fisherman on the coast a series of campaigns in which "the dead are many and the living few," the toilsome cultivation of sterile soil, the prevalence of diseases due to exposure;—these are reasons sufficiently potent to account for the strain of subdued sadness in Icelandic manners. It is no wonder that the Icelander is deficient in humour. Life to him is a tragedy, the comic element is crushed out in the stress of a hard won and hard kept existence. One noticeable incident in this prevailing sombreness is the absolute indifference to sport in almost any form.‡ Not only do the Icelanders think fishing with the rod a ludicrous waste of time and patience when a net would serve, not only do they view with

* Leopardi, "Dialogue between Nature and an Icelander."

† In one particular Leopardi is wrong, the houses are not built wholly of wood, but mainly of turf; and the risk of fire is not therefore so great as he imagines. The historical wanderings of the Icelanders to America, to Russia, and to the East in the Middle Ages, probably suggested to Leopardi the subject of his graphic sketch. On these remarkable voyages of adventure (see "Memoires des Antiquaires du Nord," 1845-1849; also "Rafn, Antiq. Americanæ," and his "Antiq. Russes et Orientales"). There seems little reason to doubt that America was discovered by Icelanders in the tenth century. Columbus is known to have paid a visit of inquiry to Iceland prior to his successful voyage to the West.

‡ There are winter games (outdoor), a kind of hockey on the ice, for example, and (indoor) cards and chess, of which some Icelanders are very fond. Children's toys are not to be found in the shops, and are rarely met with save in the houses of the Danish settlers.

dislike shooting for mere sport, but they even look, for example, upon hill-climbing, for its own sake, as a singularly futile form of British eccentricity. The idea of going up a hill merely to see the view and then to come down again, apart from any ulterior object, such as going to catch a stray pony or shepherd a flock of sheep, is to them not so much ludicrous as pitiful. It is not that they are mere utilitarians. Their tranquil mental balance, which some are apt to mistake for slothfulness, indisposes them to eccentricity in any form whatever. They are a nation of typical aristocrats, slightly *blasé*, so far as concerns volcanic eruptions, geysers, and the rumbling of earthquakes.

The population of Iceland is about 70,000.* Of these, rather less than ten thousand live in the little towns round the coast, the remainder live inland in farm houses, in the valleys, or on the margins of the firths. There are no towns inland. The farmers or bonders, as a rule, own the land they cultivate, though occasionally the farmer is the tenant of another farmer, or pays a rent to the widow of the former owner, or to a merchant in one of the towns. There is, however, no landlord class. The land, as a rule, belongs to the cultivator. Many of the farmers live now in the same valleys, and sometimes even cultivate the same land, as did their own progenitors at the colonisation of Iceland a thousand years ago.†

Iceland is among the few countries that have entirely missed the two great economic revolutions, the introduction of intensive agriculture and the introduction of machinery. Clover and the steam engine have revolutionised the civilised world, but neither of them has found a place in Iceland. The Icelandic settlers who, impatient of restraint under the Norwegian kings, betook them-

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* The population of Iceland has been decreasing during recent years. The chief cause of this diminution is emigration. Many thousands of Icelanders have found their way to Winnipeg, where they have formed a new Iceland in a region more capable of maintaining a large population than is Iceland. In 1885, the population was 71,613; in 1886, 71,521; in 1887, 69,641; and in 1888, 69,224. A large proportion of the decrease is in the population between 15 and 20 years of age. Stjórnartíthindi fyrir Island, 1889; Reykjavik, 1890. (Icelandic Official Statistical Year Book.)

† "The Dane Gardar, of Swedish origin, was the first Northman who discovered Iceland in 863. Only a few outplaces of the country had been visited about seventy years before by Irish hermits. Eleven years subsequently, or in 874, the Norwegian Ingolf began the colonisation of the country, which was completed in 60 years."—"Rafn, Antiquitates Americanæ."

selves in the tenth century to the new land, divided the country among them as the Danes did a portion of England. The particulars of this division are fortunately extant in the Landnámábok, "The Book of the Taking of Iceland," a series of remarkable Sagas in which the division of the country is described.* Practically the whole of the habitable regions were divided among the settlers, although there remained, and still remain, extensive tracts of common land, the existence of which in England led to the incidents associated with enclosures. Nor was there any serious inequality in the distribution of the land. The heads of families among whom the land was divided were chieftains of not very unequal powers.

The houses in the towns are built after the usual Danish and German pattern, usually of wood, though sometimes of stone; but in the interior the farmhouse is of turf. It is really a developed burrow. There is first a long passage with turf walls on either side, usually littered with saddles and other gear. At the end of this entrance chamber a passage to right or left, or both, leads to the guest rooms, while opposite the entrance door there is the door of the living rooms of the farmer's family. Here is the kitchen, often used solely for cooking in, and occupying the centre of the house.† In the kitchen is placed a stove situated beneath a hole in the roof through which the smoke escapes. Although the conditions are not those of comfort, nor of excessive cleanliness, yet in Aberdeenshire, in the Highlands of Scotland, and especially in the Island of Lewis, one may find hovels compared with which the poorest farmhouse in Iceland is a model dwelling. Beyond the kitchen there are sleeping rooms. The guest rooms consist of a parlour and a bedroom; both are usually very scantily furnished, for articles of furniture too large to be carried in pieces on pony back cannot be brought from the coast. The bedroom

* See Maurer, "Beiträge zur Rechtsgeschichte des Germanischen Nordens," p. 26, *et seq.*, and Maurer, "Island," p. 36, *et seq.*

† Probably a survival in domestic architecture of the hearth cult, the hearth being the altar in the worship of ancestral spirits. *Cf.* Gomme, "Village Communities," p. 129. The economic groundwork of this cult is obvious. The house was the "fire-house," the "*búr* in which the wife dressed the food." (Maurer, "Island," p. 434.) See also Dasent, "The Story of Burnt Njal," I., s. xcviii., *et seq.* The farm in Icelandic is called the *jórd;* the enclosed fields (the fenced homestead of the Saxon), the *tún;* the farmhouses, *bæir.*

contains, as a rule, two beds, each capable at a pinch of holding two persons.

The hospitality of the Icelandic farmers is unbounded. Formerly they did not accept any payment for lodging, but the increase of wayfaring during recent years has compelled them so far to modify this hospitable practice as to charge the actual cost of entertainment. This usually amounts to a mere trifle, and the attention of the host and his family is, as a rule, as unremitting as the fare is excellent. Sometimes the churches are available for sleeping places when a large party is travelling, though this privilege was for a time withdrawn owing to the boisterous conduct of some English travellers a few years ago. Occasionally the house of the *prester* is the recognised lodging-place. Wherever one lodges, one is not too fastidious after a hard day's ride, and an eider-down bed and wholesome fare are very satisfying. The farmer and his family live largely upon mutton, which they cure in vinegar for their own use. They also use whale flesh cured by the same means, extremely substantial fare, in external appearance like cut Ayrshire cheese, in taste like nothing under the sun; and for the rest, they use mainly flour in various forms, sago, cheese, dried fish, coffee, in the making of which they are *facile princeps;* cognac, which is used by custom at every course, and Danish beer. When a party of travellers arrives at a "by" or farmhouse, the farmer usually soon appears to welcome them. If a friend and fellow-countryman happen to be of the party, he salutes him with a hearty but very dignified and serious kiss. Soon after the guests have been seated in the guest chamber, the farmer's wife appears with a tray laden with coffee, and takes each guest by the hand bidding him "Skol," welcome or good health, for the word is used in both senses. During the short interval of waiting, the farmer's wife has roasted and ground the coffee beans, and infused the coffee in a muslin bag suspended in the coffee-pot. The liquor is not allowed to boil. Even in Iceland, coffee varies with the skill of the cook; but, as a rule, it is delicious. The secret is no doubt partly due to the water; but the Icelandic women have somehow acquired the art of making an excellent cup from very indifferent beans. Life at a farm is very simple. In the long winter the farmer and his family read or tell stories, or doze over the fire, mend saddles and clothes, and take out hay for sheep or ponies. In the short summer, the chief work is the ingathering of the hay crop. Sometimes the most fertile land round a "by" is broken into hummocks

a couple of feet high and a couple of feet across at the base. These hummocks are positively "shaved" by the scythe. Every blade of grass is cut, and dried, and stored in the hayloft. Every scrap of cattle and other manure is saved for the grass, and not a weed is to be seen, for grass is the only crop the Icelander can hope to reap. There are no cultivated cereals of any description. Potatoes and cabbage are grown in a small high-walled garden at the "by." The farm implements are old-fashioned, but not altogether archaic. Men and women work in the home meadows together. The one expensive commodity in Iceland is personal service.* There are but few landless persons, and the farmer must, as a rule, cultivate his farm himself with the aid of his family. It is thus a matter of economic importance to him to have a considerable number of children if his farm be extensive. Some families are very large. The size of the farms varies considerably; but a farm of normal size in one of the northern valleys may extend over many thousands of acres. Much of this land may be under perpetual ice, some of it may be inaccessible owing to precipitous bluffs, or may consist of valueless stony wastes. The unit of measurement is the "hundred"—not a measure of superficial area, but a measure of value.

The Icelandic standard of value was wadmal, a kind of woollen stuff, and the expression "hundred" applied to a hundred ells or rather a hundred and twenty ells of that material. As applied to land, a hundred means pasturage, sufficient for a milch-cow or six ewes with lambs. Even yet the use of coin is extremely limited, and is confined mainly to foreign trade and to the coast towns. Barter is the rule. Credit is given by the foreign merchants who take out goods all through the summer and receive in exchange ponies, sheep, and fish at the respective seasons.

The people do not use their family name. A man is known always by his own Christian name, and by the Christian name of his father; thus, a man whose family name was Thordàl, would not

* The rate for a guide is six shillings per day. A lamb in mid-summer is worth about two shillings. Travellers visiting Iceland during the brief summer often inconsiderately grumble at what they regard as the exorbitant sums sometimes asked by guides. When the conditions under which the people must live are realised, it will be seen that the spirit of greed is probably not so much on the side of the Icelander as on that of the tourist, who wants a great deal of his brief summer working time for as little as may be. Even Sir Richard Burton is not guiltless of this Philistinism. (See, *e.g.*, "Ultima Thule," II., p. 214.)

be called Thordàl; but would be called, for example, Pieter Magnússon, his father's name having been Magnús. A woman, too, is known by the name of her father; for example, the sister of Pieter Magnússon would be known as Guthný Magnúsdóttir. By old Icelandic custom a woman does not take the name of her husband when she marries, she retains her own name, though this does not necessarily imply latent Ibsenism. The custom, however, has been partially broken during recent years. The women of Iceland have been represented recently by a writer, herself an Icelander, as being ill-educated. I should not pretend to dispute the accuracy of the statement in general; but I think it only fair to say that I did not find it so in particular. Many Icelandic women who have never been out of Iceland speak English very fairly well, know some English books, and know Icelandic literature. There is a medical school at Reykjavík where nurses are trained, and thus, at least, for one branch of the education of women there is provision made. For other branches, it is open to question whether the encouragement of home education in letters in the long winter evenings is not to be preferred to casual attendance at school in Reykjavík. It must be remembered that a school could only be available to a very small number of Icelandic girls, and only for a very short time. There is no more frequent communication in winter between the East and the West Coasts of Iceland than there is between Iceland and Europe. A system of female education in Iceland must thus of necessity be a domestic system. One custom, rare now anywhere else, must be noticed in regard to the position of women. This is the custom known in primitive marriage lore as "handfasting." A man and woman contract to live together for a year. If at the end of the year the parties agree thereto, they are married; if not, they separate without stigma on either side. The contract may be made conditionally binding from the first. It may bind the parties to marry in the event of issue, or in the event of no issue, as the case may be. The practice still prevails to some extent. The Icelandic wife welcomes her husband's guests, but does not sit at meat with them, nor does she remain to engage in conversation. Her duties are supposed to engage her in securing their comfort behind the scenes.

One very striking element in the sociological condition of Iceland is not only the absence of crime, but the positively high tone of public opinion. This is the more remarkable,

that the population is widely scattered, and that communication is always tedious, and for a great part of the year wholly impracticable. There can be little doubt that the high tone of public and private morals in Iceland is due not so much to the inculcation of any particular dogmas, for their Lutheranism is of the moderate type that prevails in Holland, Denmark, and Germany, as simply to the absence of any desire for display or emulation, and to the prevailing simplicity of life and the rudimentary development of luxury. These elements, deeply engrained as they are in the fibre of the character of the people by centuries of practically stationary industrial conditions and isolation from the great industrial and social movements, have kept the Icelander in a sense "unspotted from the world."* The facility with which any private wrong can be redressed, or any dispute settled, contributes greatly to the tranquillity of the country. The sheriffs (sýslumenn) are at once law agents and judges. The country is divided into counties; in each county is a sheriff, and he settles the disputes that occur in his district. If appeal is made from his ruling, the case is tried at Reykjavík. The sheriffs receive their legal training at Copenhagen. In the whole island there are two policemen. These functionaries are stationed at Reykjavík, and have been known to arrest foreigners for furious riding in the streets, and to impose and collect the fines on the spot—so speedy and summary is justice, and so simple is its machinery on this enchanted island. In dress, the Icelander differs but slightly from the conventional European type. He wears waterproof coats from Manchester or Germany, and felt hats from London. Only in his shoes is he native and peculiar. These are made in the simplest and most effective manner. A piece of cured but untanned cowhide while soft and limp is placed round the foot. The exact shape is thus readily obtained, and a few stitches along the top of the shoe backwards from the point where the great toe reaches, and a few up the heel complete the shoe. Women's shoes are made in the same way from sealskin. Some wear moccasins, sometimes made of sheepskin. These are extremely comfortable, and are quite necessary in snow expeditions. In the towns, however, machine-made boots from Copenhagen and Leith replace the

* The Icelanders, like all peoples who have developed slowly from patriarchalism, have a rooted aversion to government, and a corresponding slavery to custom. This accounts for the mildness of the type, for the inborn conservatism, and perhaps also for the virtue, of the people.

primitive shoe. The woollen scarf tied round the waist is the invariable decoration and comforter of the Icelandic farmer. The women wear a simple gown of woollen stuff, generally black, and sometimes still on gala days wear the Icelandic bodice with its embroidery and silver filigree work. On festal occasions they wear a tall cap like some of the Norwegian headgear; but at ordinary times they wear the hufa, a black skull cap with graceful tassle bound at the junction to the cap with a silver ring. Old women wear another native cap, the skupla.

LITERATURE.

The language of Iceland is Íslenzk or Fornílslenzk. It has been called by Rask and other philologists, Old Northern; for though now spoken in Iceland alone, it was, at the date of the colonisation of the island, common to the whole of the North. The language has been preserved uncorrupted, so that the Icelanders of to-day read in the Sagas and in the Edda the tongue of daily usage.*

The ancient literature of Iceland is known to English readers through the translations of some of the Sagas by Dasent, Baring-Gould, William Morris, and Eric Magnusson; but there are many Sagas yet to render into English besides the story of Burnt Njal, the Gisli and Grettir Sagas, and the Saga of the Volsungs. The literature of Iceland, however, does not consist wholly of these old-world stories. There is a living literature. Among poets there are Jónas Hallgrímsson, and Matthias Jochumsson. The "Ljódmæli" of Jochumsson, published at Reykjavík by Kristjáns Ó Thorgrímssonar in 1884, contains some charming lyrics and stirring patriotic poems. His versification is exceedingly musical, and often very ingenious; this, for example, from "Sigling Ingólfs"—

Northar enn!
northar enn!
Látum gamm
geysa fram!
Stormur hár
stendur thrár
Noveg frá.
Flýjum thá
fjón og thraut
frjálsa braut,

* See "Memoires des Antiquaires du Nord," 1845-1849, p. 8.

thar til Thór
thróttarstór
ræthur einn
rétti lands ;
thangath beinn
brautar-vegur
œskilegur
liggur Ingólfs landnáms-manns.

It is curious that a very large proportion of Jochumsson's verses are addressed to persons, and that they are nearly all poems on certain occasions. This undoubtedly points to a lack of spontaneity, though it would be a mistake to suppose that Jochumsson is a commonplace versifier. His literary sympathies may be gathered from his translations. These are for the most part from Longfellow, though some are from Shakespeare, Petrarch, Goethe, Schiller, Cowper, and from modern writers, such as Bjornstjern Bjornson and Henrik Ibsen. His translation of Longfellow's "Hammer of Thor" has been set to music by Mr. Sveinbjörnsson, an Icelandic composer and musician settled in Edinburgh. The following is the first stanza :—

Eg em Thór rammi
ógnar-goth tíva
ég em Thór thrumu-goth ;
hér a eg heima
herborg og vígi
aldir og æfi.

Jón Olafsson is known as the author of "Söngvar og Kvœdi." The most voluminous recent Icelandic writer was Jón Sigurthsson, who translated the Sagas into Danish, and wrote a very large number of papers for learned societies. Besides being an industrious author, Sigurthsson took an important part in the politics of Iceland.* Among scientific writers Dr. Hjaltalín of Akureyri is best known. The article on "Iceland" in "Chambers's Encyclopædia" is from his pen.

A considerable number of English elementary scientific books are translated into Icelandic either in whole or in part. Among these are Balfour Stewart's "Elements of Physics," Roscoe's "Chemistry," and Geikie's "Geology." Several of Shakespeare's plays have been translated—"Hamlet," "Othello," and "Romeo and Juliet"—by Matthias Jochumsson, and "King Lear" by

* See "Jón Sigurthsson, the Icelandic Patriot: a Bibliographical Sketch." Reykjavík, 1887.

Steingrím Thorsteinsson. Jochumsson's translation of "Hamlet" is extremely vigorous and faithful. Sometimes he adds even a crispness and music of his own, as in his rendering of the line

"To die,
To sleep, to sleep, perchance to dream."
Ath deyja,—sofa—sofa—dreyma kannske?

A novel by Jón Thóroddsen has recently been translated into English. There are three newspapers in Reykjavík, two in Akureyri, and one at Eskifiord. Those published in the capital are called "Isafold," "Thjóthólfur," and "Fjallkonan." They are about quarto size, four pages, and cost about four kroners per year. They contain leading articles, local and foreign news, besides poems and other literary contributions. One contains a *feuilleton*, which is so printed that it may be cut from the newspaper and bound separately. The "Isafold" contains the gazette notices. The "Nordanfari" and the "Nordlingur" are published at Akureyri, and the "Skuld" at Eskifiord.

Besides Burton, Dasent, Baring-Gould, Morris, and Magnusson, Konrad Maurer, in Germany, and Professor Fiske, in America, have done much to make Iceland known to the rest of the world.

The Icelandic tongue is particularly copious in synonyms. It is noteworthy also that, considering the mildness of the manners of the people, the fertility of the Icelandic vocabulary in strange oaths, is positively amazing. The most amicable conversation is reinforced by observations so apparently violent that one is at a loss to understand it until one realises that the whole force of the national character is expressed in speech, and that there is none left for exercise either in physical violence or in industry.*

POLITICS.

The political history of Iceland has been written by Jón Sigurthsson, Sigurth Guthmundsson, and more recently by Dr. Hjaltalín. Hith íslenzka bókmentafélag (the Icelandic Literary Society), founded in 1816, has done much good work in preserving the national memorials. The Royal Danish Society for preserving Ancient Northern Literature is also doing much in the same direction.

* An instance of this conversational violence may be quoted from Thóroddsen's "Piltur og Stúlka" (Lad and Lass), translated by Arthur M. Reeves, the novel referred to in the text. A mother calls her child pleasantly a *krakkaormanganóruskinnsgreyith*. This jaw-breaking word means simply spoilt-child-worm-urchin-prank-playing-plague-of-my-life.

The struggle of Iceland for independence is a little known chapter in recent political history.* In 1830 Frederick VI. restored, in a fashion, parliamentary government to Denmark by establishing four provincial Assemblies. One for the Islands, including Iceland, met at Roskild in Denmark, and the others in Jutland, Sleswig, and Holstein. This measure led almost immediately to an agitation in Iceland for a separate Parliament to hold its sittings in the island. In 1837 this demand was so far met by the appointment of a committee of officials who were to hold a meeting in Iceland every second year for the discussion of Icelandic affairs. Apart from the national sentiment of independence, the mode of government adopted by the Danes resulted in practical subjection. The trade of Iceland was strictly confined to Danish merchants, and many obstacles were thrown in the way of the development of the country. From about the year 1830 onwards the Icelandic students in Copenhagen, under the guidance of Baldwin Einarsson, and later of Tómás Sæmundsson and Jón Sigurthsson, continued to agitate for the granting of a constitution to Iceland.† In 1840, the year after his accession, Christian VIII., so far yielded to the Icelandic demands as to institute a representative Parliament, which, after a long discussion as to its place of meeting, met for the first time at Reykjavík in 1845. Many important measures followed the re-establishment of the Althing. By 1854 the island was opened to foreign trade, the Latin school was moved to Reykjavík and was reformed, and a theological school, and later a medical school, were founded. Still the Icelanders did not feel satisfied, they continued to feel the yoke of the Danes, though much of the material discomfort of it had been removed. When Frederick VII., in 1849, promulgated his famous Grund-Lov or Constitution resigning the absolute powers which the people of Denmark had entrusted to Frederick III., the Icelandic patriots in the Althing held the doctrine that since, by the constitution, the Danes resumed the rights they held

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* The early constitutional history of Iceland is sketched, for example, in Burton's "Ultima Thule," and is fully treated in Maurer's "Island." The present notes relate only to recent history. For a popular account of this see, for example, "The Story of Denmark" (Longman's), and the excellent article on "Iceland," by Dr. Hjaltalín, in "Chambers's Encyclopædia." I have to acknowledge the kindness of Mr. Jón Jónsson, of Reykjavík, in giving me many hints.

† See Jón Sigurthsson, *op. cit.*

before the absolute monarchy was established, the Icelanders were also entitled to resume the rights they held. And these rights they interpreted to mean entire freedom from the control of the Danish Parliament, the island owning allegiance only to the king, who was now to act as a constitutional monarch. Prior to the promulgation of the constitution, the Icelanders had been pressing these views upon the king, and had especially made the following demands :—

1. That the Althing should have the same powers in Iceland as the Danish Parliament had in Denmark.

2. That a Governor responsible to the Althing should be appointed to reside in Iceland, and, in addition, a minister to reside in Copenhagen.

3. That the finances should be separated from those of Denmark, and that Iceland should then share in the national expenditure.

In 1851 an attempt was made to limit the powers of the Althing to harmonise with those of the local government bodies in Denmark, and to provide for the representation of Iceland in the Danish Parliament by six members, the Parliament at Copenhagen and not the Althing at Reykjavík to have the power of levying taxes. This, however, was set aside, though the finances of Iceland continued to be administered by the Danish Parliament. The relations between Iceland and the Parliament in Copenhagen were so strained that the Danes themselves became really anxious to find some way out of the difficulty, especially since the budget of Iceland was in a chronic state of deficiency. But the Icelanders were extremely obstinate, they disputed altogether the right of the Danish Parliament to interfere in their affairs. The Danes even offered to pay an annual sum to Iceland; but this was rejected because the Althing refused to settle the subject piecemeal. A bill defining the constitutional position of Iceland was laid before the Althing in 1871 by the Danish Government; but the Althing refused to entertain it, and notwithstanding the passing of the measure by the Danish Parliament in 1871, the Althing declined to recognise its validity. Apart from the determination of the Althing to have nothing to do with the Danish Parliament, which they regarded as a local Parliament of the same standing as the Icelandic Althing itself, the Althing has had a controversy with the king as regards his power of veto. A measure requires to be brought thrice before the Althing before it is sent to the king for his signature. He may withhold this if he

pleases, and on occasion has done so. This the Althing holds he has no power to do. The more Radical members are understood to go so far as to declare that the Althing might pass a measure separating the island entirely from the Danish crown and expect the king to sign it. This is, of course, an extreme view; but it points to the conclusion that what underlies the whole Icelandic question is the desire for separation from Denmark. Iceland is in a peculiar position. Formerly belonging to Norway, from whose people the Icelanders are descended, the island came into the hands of the Danes, not by conquest of Iceland, which was never really conquered, but by the temporary hold of Norway obtained by Denmark. Thus the Icelanders naturally look upon the Danes as an alien people, and have slender racial or other sympathy with them.* The economical development of Denmark, too, has gone on, while Iceland has been neglected. It is true that Denmark has not reaped much material advantage from so poor a country, but the blame even of this lies upon Denmark herself, for the trade of Iceland has been restricted, and the control of the public funds so managed that nothing whatever remained for roads, bridges, harbours, or any means of developing the country. Recently, however, the Government has begun the construction of a road to extend across the island from Reykjavík to Akureyri, by Geyser and the Blandá Valley. Iceland has for the past fifty years, as we have seen, been a thorn in the side of Denmark. The situation just now is that of an imperative demand on the one side for complete autonomy, and on the other extreme reluctance to grant it. The matter comes up again before the Althing this year (1891). In 1889 consideration of it was staved off by a diplomatic move on the part of the leaders of the patriotic party; by the next meeting of the Althing some compromise may probably be arrived at. Among leading Icelandic politicians may be mentioned Mr. Skúli Thóroddsen and Mr. Benidikt Sveinsson.

TOPOGRAPHICAL NOTES.

On approaching Iceland from the south, especially if one nears its wild coasts in the evening, the weird and sombre aspect of its

* The social friction between the Danes and the Icelanders would no doubt be mitigated, if not removed, by the settlement of the constitutional question. At present neither people fully understands the other, and the memory of past oppression rankles in the minds of the Icelanders.

mountains, enveloped perhaps in dense masses of cloud, its unfamiliar incongruities of form, scarred and seared as it has been by ages of frost and fire, impress the imagination with the unique physical character of Iceland, perhaps even in a greater degree than it may have been impressed by Icelandic myth and romance. The mountains that first come in sight are those of the Oræfa Jökull, looming away to the north in black shadow, and later, and nearer but still distant, Myrdals Jökull, a vast pile of gloomy and desolate volcanic rock. From Myrdals Jökull and from its neighbour Eyjafialla Jökull there are thrust down to the sea fantastic tongues of land sometimes almost insular, and frequently ending in sudden headlands like pickets thrown out from the advance lines of an army. Ceaseless grinding of the screw in a glassy sea, with flights of sea birds following in our wake, brings us in sight of the Westmann Islands, a numerous group of singularly diverse shapes. Only one of the islands is of any considerable size. Upon it is the little village of Heimay with its church, situated in a gap between two ranges of hills. A little to the north of this gap there is a narrow strait dividing the largest island from one of the smaller, and we steam through this channel between precipitous cliffs white with guano and haunted by myriads of sea fowl. We were told that the fisher folk who inhabit the island kill the sea fowl and use the dried oily bodies for fuel in the winter. The cliffs are not high, and are by no means destitute of verdure on the top, to which the sheep are raised by means of ropes. We passed the cliffs and the outlying Westmanns, and all the long evening ran by the coast of Iceland. A strangely irregular coast it is, high lands and flat lands curiously alternating. Sometimes we find the mountains sending their spurs into the sea—sometimes a long low peninsula rising only a few feet above water level and not many yards broad, barely distinguishable from the deck, yet running for miles parallel with the coast, leaving a long narrow inlet between, and forming an immense natural breakwater. Towards midnight we neared the peninsula of Gullbringusysla, which forms the southern shore of Faxafjörth, and the sun went down behind its long low hills.

The sunsets of Iceland may not be described nor even painted; the wealth of colour, the "supreme great glory" of the evening, as the sun set night after night in Denmark Straits, is to be remembered vaguely as a dream that never yet was realised on any canvas. Before sunset on this first evening in Icelandic

waters a scene of great beauty suddenly burst upon us. The western cloudland cleared away and disclosed at an immense distance seaward what at first appeared to be a phantom island. It sprang so suddenly from the western haze, and raised its volcanic peak so high into the heavens, that if we had been told that it was marked on no chart, and had no tangible existence save as a haven for the Flying Dutchman, we should have been believing and unsurprised. The apparent island was Snæfells Jökull, ninety to a hundred miles away, snow-covered from the top of its truncated cone to the horizon, white and wonderful in the clear northern air. Like any other magic island, it had no visible connection with any terrestrial thing; but as we steamed north-westwards, peak after peak of the high and rugged table land of Snæfellsness rose in jet blackness out of the water, for it was near midnight, and the sun had gone down behind them; and then we saw that Snæfells Jökull stood as a snow-covered sentinel at the uttermost end of a long peninsula, and that all the breadth of the Faxafjörth lay between us and it. We ran along the coast in the night by Reykjanes, passing the Fire Islands, the scene of not very remote volcanic action. At Reykjanes or the "Smoky point," there is the only lighthouse on the coast of Iceland, and as if the nature powers that claim the island for their own had wished to demonstrate the futility of attempting to control them by any of the modern arts, the rock upon which the lighthouse is built was a few years ago cleft by a volcanic upheaval, and the base of the lighthouse was displaced.

By dawn of the morning we had rounded the point, run up Faxafjörth, and dropped anchor before the capital in Reykjavík Bay. The bay, with Esja towering above on the right, and the broadly dignified bulk of Snæfells Jökull seventy miles away, forming its background toward the sea, is certainly beautiful, though one's first impressions of the coast are those of arid and depressing bleakness. Reykjavík is a town of about 3,500 inhabitants.* The Althinghus or Parliament House, the Cathedral, the Latin school, the Governor's house, and three inns, are the public buildings. Reykjavík has sprung up around the fishing factories. Here the French schooners, that carry on the bulk of the fishing round the coasts beyond the three-mile limit,

* The population in 1888 consisted of 1,621 males, and 1,978 females; total, 3,599.—"Stjórnartíthindi fyrir Ísland," 1889, c. 9, 35.

come for their supplies; and here the farmers, within a radius of about a hundred miles, bring their sheep and ponies for exportation, and carry off in exchange flour and other provisions for winter use. The houses in Reykjavík are mostly of wood, some are of turf, some of concrete with roofs of galvanised iron, a few are of stone. The city, as the Icelanders like to call it, is not specially imposing; yet the ample square opposite the cathedral, the trim business places, general stores, an apothecary's shop, some half-dozen schusmather, several jewellers, a bank, and some three or four publishing houses, give the place a certain air of grandeur out of proportion to the pettiness of its size in southern eyes. Breakfast at Zœga's, dinner at Halberg's, are *de rigeur*, and both are delightful. Icelandic salmon, with its piquant flavour, and coffee to be remembered afterwards with unappeased desire, are Zœga's delicacies—then ponies and a ride. The country round Reykjavík consists of stony plains, lava fields and bogs, not a tree nor a shrub breaks the lifelessness of the scene. There are no roads here nor anywhere in Iceland, save one now being made from Reykjavík to Thingvelli, a section of the new Government highway. The tracks, however, afford fairly good going. One canters along in the wild exhilarating canter of a good pony, as sure-footed as fate, and with an uncommonly eager desire to get to the end of its journey with the utmost despatch. Splashing through rivers up to the saddle girths (for bridges are few and far between), clattering over the stones, threading one's way among grassy hummocks or over bogs, circumspectly treading a narrow shelf on a mountain side, or scampering across lava sands and throwing up clouds of fine volcanic dust that fills the hair, the eyes, the ears, the nose, and the interstices of the teeth; one forgets that there are overgrown cities and railways and progress and never misses them. These very experiences endow Iceland with an unrivalled fascination for those who like for a while to get into touch with the wild moods of nature.

The way of the country is for each traveller who purposes a journey lasting longer than a single day, to take, that is, buy or hire, at least a couple of ponies for himself and one or more for his baggage; he will also have to take a couple of ponies for the guide, and if the party is a large one, another couple for a man who will attend to the horses, besides any ponies and spare ponies that may be required to carry a tent, cooking apparatus, or other baggage. If a party contemplate a journey of a few days

in the interior, their progress is thus a kind of triumphal gallop of a considerable cavalcade. The spare ponies run in front without either saddle or bridle, and occasionally are seduced from the straight path, which is not a path but merely a way, by the salutations of their friends or by a whiff of fresh hay. The track is sometimes so narrow that there is room in its width for only a single horseman; sometimes one crosses a plain on which an army corps could perform evolutions if it did not get lost in the cracks. Furious riding is very properly forbidden by law in Reykjavík and the other little towns; but elsewhere it is inculcated by custom. No Icelander will take the trouble to mount unless there is decisive need for it; but when he is mounted he goes for his objective like an arrow at a mark, and when he hits it he is off his horse like an automaton. The Icelanders do not use spurs, though some use a very small and very blunt spike in the heel of the boot. The fact is, unless one gets a screw, and there are such in Iceland, the trouble is not to make your horse go, but to get him to go at a rate which will enable you to see the landscape with a reasonable degree of leisure. One arrives at a slight gradient, and finds the limitless expanse of mountain upon mountain, and the river in the valley beneath one broadening out into lagoons on a bed of black lava sand, or gets a glimpse of a golden-winged plover poising on the stone snowposts that mark the track in the winter, and being in no hurry for a day or two, for time is not, in these regions, one might wish to linger a little, when suddenly the spare pack in front, being familiar with the view, and having private visions of the next grazing place, takes into its head the idea of an impromptu Derby, and join in it you must pell-mell. It is difficult to give a passably fair notion of the festively rollicking character of an Icelandic ride. You may meet a party with pack ponies returning laden from the coast, or going down empty with the panniers loosely flapping on the ponies' flanks. The pounding of the hoofs of your troop on the volcanic tufa makes the poor drudges prick up their ears, and away they gallop, wobbling their panniers from side to side, finally tossing them high in the air to the grief of their juvenile herd, who urges her steed after them, and ultimately heads them off among the lava rocks. Or one's rollicking spirits may be depressed by meeting a troop of another and wierder sort. Over the lava plain, perhaps for a whole day or more, there journeys, for the last ride, a coffin on horseback with a score of sober Icelandic farmers riding to the

quiet graveyard at Reykjavík, beside the little church behind the town.

The Icelandic bridle gear is unique. It is generally made of heavy brass castings of simple but fine design. These castings were formerly made in Iceland, but are now made in Denmark from Icelandic patterns. Stirrups are also of fine design, often open in character, not unlike some Japanese sword guards. The Icelandic whip is a dainty weapon. It is always formed of malacca, and is frequently silver-mounted. The thong is nearly as long as that of our dog whip, for it is rarely used on the horse one rides, but on the spare horse in front. If the leader runs, no pony that has the faintest spark of Icelandic pride would fail to run his hardest to keep up with or supersede him. Nothing is more astonishing than the zest with which the ponies race each other, or than the marvellous way in which their dainty feet find a track in ground that would knock up an English hunter in half an hour, or than their staying power, for one will carry you over such ground forty miles in one day, and run before you while another carries you other forty the next. An Iceland pony on its native lava and pumice is the nearest approach to a Hounonym one is likely to find in the horse kind.

Walking is an impossibility. There is a story of a self-confident walker who set out from Reykjavík to walk to Hekla. The next day a party on pony back saw at a distance what appeared to be a man's hat lying on the ground and realised that a handkerchief was being wildly fluttered by some agency beneath it. Tying a number of scarves together to serve as a rope, one of the party made his way cautiously on the yielding surface of the bog to the point where the unfortunate walker was found immersed to the chin. He had been there for hours; he was rescued with difficulty, literally scraped, put to bed, and enabled to pursue his journey by the recognised Icelandic means of locomotion. The treachery of an Icelandic bog is inconceivable. A fair surface of turf may suddenly give way beneath one, or a step upon a green sward may set it for many yards round into billowy motion. A guide, a good horse, and excessive caution, are needed where cracks in the lava have left a crevice in which yielding mud has formed a bog.

Undoubtedly the finest aspect of Iceland is the coast line from the sea. From Reykjavík round the north coast is a sail of the most enchanting beauty. Gradually as the sun sinks towards

midnight behind the hills of the long peninsula which forms the northern boundary of the Faxafjörth, we crept up under the very crest of Snæfells, looming larger and larger before us until we passed Öndvartharnes, saw the broken rim of the great crater and the inaccessible summit,* and steamed away to the north across the waters of the Breithifjörth. Rugged terraces of trap worn by sun and snow, with the detritus in slopes beneath them, form the coast line of the West Firths. Each one of the six greater firths is marked off from the other by a bold headland, and away inland a snow mountain, the Glamu Jökull appears above the tops of the lower range. Small farms with green patches and scattered flocks of sheep are seen in the sheltered nooks under the shadows of the mountains. Round by bays and inlets, past great promontories haunted by myriads of sea birds, and with rugged and fantastic outlines suggesting all sorts of resemblances, we reached the Horn, the North Cape of Iceland, not the northmost point, but the most northerly of the great north-west peninsula. Only one hundred and eighty miles to the west lies Greenland, but the warm July air, and the perfect calmness of the water, with divers ducking under our very bows, suggested anything but the Arctic Sea. Yet Denmark Straits are filled from shore to shore in the winter with ice, and even in summer the passage cannot always be made. The scenery from the Horn eastwards to the broad arm of the sea, the Hona Floi, is not so deeply indented as is the west coast, but it has features of great beauty. Beyond the ragged promontories which afford many picturesque views, there is stretching along a great part of the coast, the magnificent Dranga Jökull. The Dranga is really a snow-field, at least eight times larger than the largest of the Swiss glaciers. It fills almost the whole of the extreme north-western neck of Iceland, and suggests resemblance to the great glaciers on the coast of Greenland; cracked in places with enormous chasms, rounding off the hills with dazzling white and sending down its ice-foot into the sea, there is a certain solemn Icelandic restfulness in its grandeur. From off the Dranga, we steamed across the Hona Floi and into the Hona Fjörth to Blönduós. Near Blönduós is the Hop, the lake of the men of Willow-dale, and at the foot of the Hona Floi is the Middle Firth and Biarg, all famous in the old stories.

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* Henderson gives an interesting account of his attempt to reach the summit.—"Iceland," p. 315.

Blönduós is a small station at the mouth of the Blonda or Blandá, the white river. It consists of some five houses underneath a raised beach. The existing beach is of black lava sand, forming a strange contrast to the river white with the mud of the glaciers of the Arnarfells Jökull. In the river mouth about the sharply defined line, fluctuating with the currents and the tide, where the white waters of the river meet the darker waters of the Hona Fjörth, there sported a number of seals, who thrust their cat-like heads and mild faces a few boats' lengths from the surf boats that took us ashore. Towards evening the Hona Floi was darkened by great masses of cloud, and wind and rain brought our first experience of bad weather. The cargo was landed with difficulty, and the coals, bags of flour, boxes of raisins, candles, and all sorts of miscellaneous goods for domestic consumption were piled on the beach, where the country farmers came with their pack ponies and carried off their winter provisions. The next inlet to the Hona Floi is Skagafjörth, one of the most beautiful firths in Iceland. The firth is lined by precipitous headlands, and in the centre of it there rises the abrupt square rock, Drangey, where Grettir lived as an outlaw, and where he was slain by Thorbiorn the Hook. Drangey is divided almost in two by a chasm which appears as one approaches the island at a certain angle, clearly the result of one of the upheavals which have left their marks upon almost every foot of land in this fiery region. Malmey is another island in this firth, less impressive than Drangey, but rising also abruptly out of the sea, a long rolling tableland. At the head of Skagafjörth is the town of Sautherkrok, where there is a station for drying fish carried on by a hospitable Dane, whose pleasant drawing-room and lively family are unexpected delights on a shore where one finds an ice-boat as a matter of course. Sautherkrokr is situated at the foot of a most perfect and regular example of a raised beach. The plateau behind the town is about 100 feet above the present sea level. From the cairn on the top of a cliff, a spur of Tindastoll, the long mountain which forms the western boundary of Skagafjörth, there is a most charming view of the inlet. The sail from the firth out into the Arctic Ocean and round Hifstángi and Langanes is interesting, but not so indescribably grand as that round the north-west coast. Here we passed into the Arctic Circle, had a glimpse of Grimsey, a solitary island thirty-five miles due north from the mainland of Iceland, and saw away beyond, the ice-blink, infallible sign of polar regions.

At Langanes it is said that the sea swallows, which abound on the coast, and even on the rivers far into the interior, collect in one vast flock prior to their flight southwards for the winter. From Langanes we also fare southward by different means of locomotion, but we stay by the way to sail up Seythisfjörth, a beautiful firth about twelve miles long, the upper portion being quite out of sight of the sea. The rocks on one side of the firth are like wild castellated ruins, on the other side the mountains are higher, and are dotted with glaciers. Here, a few years ago, an avalanche from the crags cut the village in two. Within a few hours' ride of Seythisfjörth, there are waterfalls and other natural wonders that may one day attract a stream of tourists. Seythisfjörth is easily accessible : it is under three days' sail from Leith in fine weather. There, as in most of the Icelandic ports, the fishing is in the hands of a Dane who buys his fish from the Faröese, who, being subjects of Denmark, fish inside the three-mile limit, and from French fishermen who fish outside. Here also may be seen occasionally a Grimsby trawler, which has sought the shelter of the fine natural harbour.

While one may thus form an impression of Iceland from the deck of a steamer, may inhale its bracing air, and witness the ruggedness of its contour and the dazzling whiteness of its snow mountains, the "true inwardness" of the country can only be realised by journeying in the interior. There only may one fully enjoy its indescribable fascinations, may understand the reason of the settled sombreness of Icelandic manners, may be transported if one chooses backwards for a thousand years, and may see after all, in no very faint picture, the manner of life that Grettir and Gisli, Sigurd and Gunnar, and the other heroes of the old stories must have lived. Nothing strikes a Southerner so much as the treelessness of Iceland. There are no pines on its mountains, there are even no shrubs in its wind-swept valleys. The flora is Alpine. The commonest plants in the plains and on the slopes of the hills are the graceful silvery-leaved lady's mantle (*alchemilla alpina*), very small but very beautiful; sea-pink, very copious and large, lending even a brightness of colour to the stony wastes; lady's bedstraw, grass of Parnassus, dwarf-willow, spotted orchis, and many other wild flowers familiar in our own Highlands. In the upper regions there is an immense variety of mosses, including, of course, the Iceland or reindeer moss, which is found at a height of 2,000 feet and upwards Though nothing grows tall, there is

really profuse dwarf vegetation up to the very edge of the snow. The grass in the "tún," or enclosed fields, is natural grass, in the sense that it is self-sown, but it is most carefully manured and cut. It resembles the Italian rye-grass grown in this country, and flourishes quite luxuriantly over and between the hummocks.

The most fertile regions are in the northern part of the island. In the great valleys there are noble rivers, like the Blandá, the Thiórsá, and the Quivering Flood, and on the banks of these rivers there are dotted farmhouses with fertile patches, where are reared the ponies and sheep which form the staple of Icelandic products. In the south-east it is said there are still some trees; and at Akureyri in the north five are pointed out by the admiring natives as relics of a primitive Icelandic forest. There is no doubt that at the time of colonisation of Iceland, there were trees, but these have been ruthlessly consumed in the course of time, and none having been planted during the process of cutting down, the needed shelter to saplings has been lost. There is, however, the not remote probability of the climate being less suitable for the growth of trees now than it was 1000 years ago. Efforts and experiments are being made with the object of introducing forestry into the country. There is little visible wild life in Iceland, excepting that of birds. The sea swallow or Icelandic tern haunts the rivers. It resembles a gull, though its form is more slender and graceful, and its forked tail gives it a unique character. Ravens are common; there are ptarmigan on the hills, and rarer the Icelandic falcon, while round the coasts there are, for example, the great northern diver, and the eider duck. There are no crows and no snakes in Iceland. There are or were foxes, and there are legends of polar bears which have crossed from Greenland on pack ice and, hungry with their long voyage, have pursued Icelanders on the coast. Wild reindeer, not indigenous, but imported in last century, occupy an uninhabited region between Askja and the eastern firths. In all the larger rivers salmon are fairly plentiful. With their characteristic indifference to sport, the Icelanders do not angle, but they net the salmon, or they rent their rivers to English sportsmen. In the firths plaice and halibut are caught by trawling.

The Icelandic year is divided very much as is our winter day—one-third sunlight, one-third darkness, and one-third twilight. Thus for nearly four months of summer the stars never shine, and for four months of winter the sun never shines. It was not

till we came a long way south of the Faröes on our homeward journey that darkness began to steal upon us and the stars began to make the night bright by an unaccustomed brightness. We had lost account of day and night, and we thoroughly understood how the supernatural flight of time, so frequently an element in fairy tales, presented no difficulty to the northern mind, for in the north a year and a day are almost convertible terms.

Even if one is disinclined to explore the awful grandeurs of the Vatna Jökull, the great volcano and snow-field in the south-east, or to essay Askja, the region of the most recent volcanic eruptions, or to undertake a two or three days' journey on pony back over the great central desert, or to climb Hekla, or to seek the haunts of the wild reindeer, or to undergo any serious adventures or hardships, he may yet have his eye filled with novel and wonderful sights by riding anywhere in the interior, or even lazily enjoying the coast from the deck of a steamer.

XI.—*Some Experiments on the Viscosity of Air.*

By James Erskine Murray.

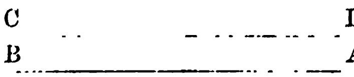
[Read before the Society, 17th December, 1890.]

[*Note.*—The descriptive part of this paper (from Section 2 onwards) was the Essay which gained the Cleland Medal of the University in the Session 1889-90.]

The following paper consists mainly of the description of some experiments and does not treat the subject of Viscosity theoretically. For this reason I think it well to preface it by a short definition of the unit in terms of which Stickiness or Viscosity is measured.

Such fluids as treacle and oil are admitted by everyone to be sticky; shipbuilders know that in designing a swift vessel they must allow for the stickiness of water; and lastly, railway engineers recognise that the stickiness of air retards the motion of a train.

To define the unit of stickiness let us suppose that we have



two planes AB and CD, of solid material, at unit distance apart, and that the space between them is filled with the liquid or gas whose viscosity we wish to find. Let the plane AB be fixed and let the plane CD be moving horizontally in the direction from C to D with uniform velocity, one centimeter per second. Now, since every fluid has some stickiness, there will be a dragging force on the surfaces of these planes. A force on CD against its motion, and an equal and contrary force on AB tending to draw it forward in company with CD. The amount of this force on unit area of either plane measures the viscosity of the fluid. The numerical value of this quantity is usually represented by μ. This number is evidently larger for sticky than for mobile fluids.

1. There are two principal methods by which the viscosity of any liquid or gas may be determined. Theoretically, the following is the better:—A disc is suspended at its centre by a fine wire

from a solid support. The disc is turned in a horizontal plane, so as to twist the wire slightly, and is then let go. Now the period of the vibrations which it would perform if the air had no stickiness can be calculated from the dimensions and materials of the disc and wire. Thus, by comparing the calculated period of vibration with that observed by experiment, we can find the retardation due to stickiness of the air. From the amount of this retardation the viscosity of the air can be deduced by a process of mathematical reasoning. This method has the disadvantage of great practical difficulty. It has, however, been adopted by many experimenters on account of its theoretical completeness.

2. The method used in my experiments was that in which the difference of pressure at the ends of a tube, of known diameter and length, is compared with the rate of flow of the fluid through it.

Suppose l cms. be the length of the tube, r cms. the radius of the tube, $p_1 - p_2$ dynes per sq. cm., the difference of pressure between the ends, and t seconds the time taken for the quantity Q c.c. to flow through. Then the value of μ, the viscosity, is calculable by the relation—

$$\mu = \frac{\pi\, r^4\, (p_1 - p_2)\, t}{8\ Ql}$$

This method was adopted on account of its great experimental simplicity. The value of the viscosity found, notwithstanding the roughness of the measurements, is within six per cent. of that given by Clerk-Maxwell. Among previous determinations the best known are :—

(1) By *Transpiration* through a tube—

O. E. Meyer,	... ...	·000184, at 14°·4 C.
Puluj,	... ...	·0001855, at 15° C.
Warburg,	... ...	·0001845, at 15° C.

(2) By *Vibrator* method—

O. E. Meyer,	... ...	·000360
Stokes,	... ...	·000104
Kundt and Warburg,	...	·000189 at 15° C.
Clerk-Maxwell,	... ...	·0001878 (1 + ·00366t)

In all cases the value is given in C.G.S. units, and in the last t°C. is the temperature of the air. Clerk-Maxwell's result is considered to be nearest to the truth. If, by means of his correction for temperature, we deduce from the various results the corresponding

values of the viscosity at 8°·8 C., which was about the temperature in my experiments, we find—

(3)	O. E. Meyer, ...	·0001803, at 8°·8 C.	
	Puluj, ...	·0001815, „ „	
	Warburg, ...	·0001805, „ „	
(4)	Kundt and Warburg, ...	·0001850, „ „	
	Clerk-Maxwell, ...	·0001938, „ „	

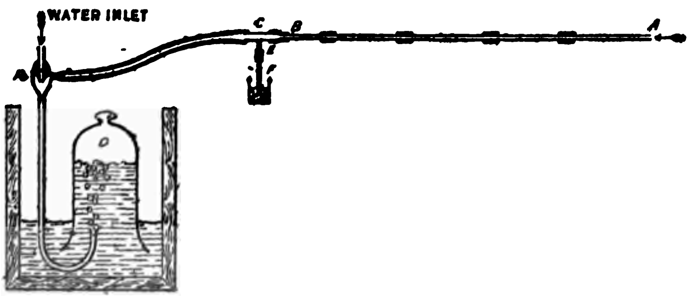
The average value found from the three of my experiments in which the pressure was quite steady all the time, is

·000183.

No account was taken of the humidity of the atmosphere, which, according to O. E. Meyer, tends to lessen the viscosity. This seems to justify the expectation that with more care the method may be of greater value than was hitherto supposed. The value of the diffusivity of air for laminar motion, found from the above by multiplying by the density, is ·1464 sq. cm. per second. A table of results is appended to the paper.

APPARATUS.

The long tube AB (309·8 cm.), through which the air flows, is of glass. (See figure.) The internal diameter, found by a volumetric



method, is ·3220 cm. It is open to the atmosphere at A. At the end B, nearest the pump P, which withdraws the air, there is a short piece of wider glass tubing, C, to which a cross tube, EF, is attached; the latter dips into a vessel of water. During an experiment the height to which the water rises in EF is a measure of the pressure in the tube C. The tube AB is made up of five pieces, the ends of which were ground and fitted carefully together, the joints being covered with thin, tightly-fitting India-rubber tubing. This arrangement was adopted because it was found

impossible to get a single tube of uniform bore, of sufficient length to give an easily measureable difference of pressure with a moderate velocity of the current. The air, after passing through the pump, is received in the belljar, D. The pump is stopped and the air is measured when the levels of water inside and outside are the same.

Sources of Error.

The most apparent objections to this apparatus are the following:—

(1) Ordinary glass tubing is generally somewhat conical. This might be overcome either by using tubes of greater diameter and length more accurately of the same bore all along; or by calibration and corrections for the differences of diameter of the various parts.

(2) The air to be measured is passed through the pump and mixed with water before measurement. It is very improbable that much is dissolved or lost, as the water already contains a considerable quantity in solution.

(3) The gauge EF might not give a true measure of the pressure in C. This was experimentally tested as follows:—The tube AB was detached from C. The zero of the pressure gauge was then taken (that is to say, the height of the column of water in EF due to capillary attraction alone). The pump was then started, causing, of course, a much more rapid flow in C than was the case when AB was attached. The gauge still indicated zero, thus showing that the short length C of wide tubing, as also any effect due to eddies at the junction of EF with it, might be left out of account in calculating the results.

(4) The pressure indicated in these experiments was not more than that due to a column of water 2·5 cm. high. The possible error of measurement with the apparatus used was about ·5 mm., that is to say, about 2 per cent. of the whole pressure. The error from this source could best be reduced by adding to the length of the tube AB, and so getting a greater difference of pressure.

(5) The air was not dry; therefore, according to Oskar E. Meyer, the value found for μ should be too low. It *is* lower than that given by Clerk-Maxwell.

(6) The tube is in joints. The disturbance caused at each joint is probably very small, as the tubes are well fitted together, and the length of smooth tube is very great as compared with the small inequality at a junction.

DESCRIPTION OF AN EXPERIMENT.

In each experiment the pump was first started. When the pressure had become constant the end of the tube in connection with the outlet of the pump was put under the belljar and the air caught; the time of doing so and the time of stopping when the jar was full enough were accurately noted. Each experiment lasted about six minutes, during which time the pressure was observed every thirty seconds. The zero of the gauge was observed immediately before the pump was started, and again about two minutes after the end of the experiment. The possible error due to want of accuracy in measuring the air transpired was about $\frac{1}{3}$ per cent. A correction was made for the temperature at which the air was measured. The barometric pressure was, on most of the days, near the standard. Before the experiments were commenced the tube was washed, and then thoroughly dried by gentle heating with a spirit flame, while a current of air, dried by means of calcium chloride, was flowing through it.

TABLE OF EXPERIMENTS.

Experiments.	Time of Transpiration.	Quantity of air transpired (cor. for temp.).	Average difference of Pressure between ends of tube per sq. cm.		Radius of tube.	Length of tube.	Viscosity found.	Remarks.
	Secs.	c.c.	gms.	dynes.	cm.	cm.	C.G.S.	(Experiments I.—V. were preliminary.)
VI.	301	3691	2·725,	2676	·161	309·8	·000186	
VII.	332	3593	2·3,	2258	,,	,,	·000178	Pressure constant during whole time.
VIII.	337	3621	2·38,	2337	,,	,,	·000185	
IX.	320	3538	2·404,	2360	,,	,,	·000182	Pressure constant during whole time.
X.	448	3566	1·8,	1768	,,	,,	·000189	Pressure constant during whole time.
XI.	469	3580	1·61,	1581	,,	,,	·000177	
XII.	421	3856	2·06,	2023	,,	,,	·000188	
XIII.	396	3566	1·846,	1812	,,	,,	·000172	
XIV.	419	3704	1·865,	1831	,,	,,	·000177	
XV.	369	3469	1·86,	1827	,,	,,	·000167	
XVI.	382	3745	2·22,	2180	,,	,,	·000189	

XII.—*On the Physiological Action of Carbon Monoxide of Nickel.*

By JOHN G. M'KENDRICK, M.D., F.R.S., and WILLIAM SNODGRASS, M.A., M.B.*

[Read before the Society, 29th April, 1891.]

BY the kindness of Mr. Ludwig Mond, we have had the opportunity of examining the physiological action of carbon monoxide of nickel, a substance of unique chemical composition represented by the formula $Ni(CO)_4$. This substance was discovered by Mr. Mond, co-operating with Dr. Carl Langer and Dr. Friedrich Quincke, and its chemical properties have been described by them.† It is a clear liquid of high refractive power, boiling at 43° C. at 751 mm., and having a specific gravity of 1·3185 at 17° C. The fluid is soluble in alcohol, benzine, chloroform, and pure olive oil. It readily evaporates on exposure to the air, the remainder, even in a well-stoppered bottle, undergoing decomposition. For this reason, it must be kept in sealed glass tubes, and we found it difficult to preserve it when the point of the tube was broken off, and a small quantity removed for purposes of experiment, however quickly we sealed up the point in a blow-pipe flame.‡

The method of administration adopted was by subcutaneous injection. The following experiments were, in the first instance, made upon frogs:—

Experiment 1.—At 11.3 a.m., 1·14c.c. injected under skin of the back. The substance evidently acted as an irritant, as the animal contorted its body and rubbed the skin over the seat of the injection with its hind limbs; at 11.30, there was paralysis of fore and hind limbs; at 11.50, respiration was very slow; at

* From the Physiological Laboratory, University of Glasgow.

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‡ A notice of a communication on $Ni(CO)_4$, by M. Hanriot, appears in *Nature* of 9th April, 1891, as having been communicated by him to the Société Chemique on 27th February. In this notice it is stated that M. Hanriot found the substance to be more poisonous than carbonic oxide, and that the blood gave the characteristic spectrum of co-hæmoglobin. Our work was carried on independently at intervals during last winter.

12.20, the animal was dead. The blood was of a bright red colour, and, when examined with the spectroscope, showed two absorption bands in the position characteristic of carbonic oxide-hæmoglobin. These bands persisted after the addition of sulphide of ammonium.

Experiment 2.—At 2.48 p.m., ·54c.c. injected under skin of back; at 2.51, the frog croaked repeatedly and rubbed irritated spot with its hind legs; at 2.53, we observed that the eyes were remarkably prominent; at 2.56, it rubbed its head with its fore limbs; at 3.12, the eyes were much retracted—the web of the foot examined under the microscope showed a very slow blood stream in the vessels; at 4.20, there was slow respiration; and at 4.22 the animal was dead. A spectroscopic examination showed two absorption bands irreducible by sulphide of ammonium.

Experiment 3.—At 3.25 p.m., ·3c.c. injected under skin of the back; at 3.31, the frog rubbed its back with its hind limbs and its head with its fore limbs; at 3.32, the eyes were retracted; at 3.46, the circulation in the web was very slow; at 3.58, there was gasping respiration; at 4.2, there was almost complete stoppage of circulation in the web; at 4.8, the animal was killed. The heart was beating feebly, and a drop of bright red blood taken from it showed the double absorption band irreducible by sulphide of ammonium.

These experiments all pointed to the poisonous action being similar to that of carbonic oxide.

Experiment 4.—At 10.33 a.m., ·3c.c. injected under the skin of the back of a white mouse; at 10.36, there was slow prolonged breathing; 10.38, convulsive twitchings of limbs, especially of hind legs; 10.39, a general spasm; 10.39½, fibrillar twitchings of muscles; 10.40, gasping breathing, lying on back, paralysed; and at 10.48, the animal was dead. On opening the body the tissues were seen to be of a bright red colour, and the vessels were full of bright red blood. The blood from the heart showed a double absorption band in the spectrum, not changed by addition of sulphide of ammonium. The animal died in 15 minutes.

Experiment 5.—At 10.43, a small rabbit, weighing 330 grms., had 1c.c. injected under skin of back. It died in 20 minutes. There was marked paralysis of the limbs, appearing first in the hind limbs. The respirations became extremely rapid and gasping towards the close of life. This was the first case in which the temperature of the rectum was taken at intervals. The first

reading, immediately before receiving the injection, was 35·5° C.; the temperature steadily fell, and at the moment of death it was 32° C. This observation directed us to the remarkable reduction of temperature caused by $Ni(CO)_4$. The next experiments illustrate this point.

Experiment 6.—A larger rabbit than the subject of Experiment 5 was taken, but the weighing was unfortunately overlooked. The animal was wrapped in a towel, and a thermometer was kept in the rectum during the greater part of the experiment. Great care was taken to keep the thermometer always at the same distance in the bowel, as it was found that pushing it a little farther in, or drawing it a little farther out, caused considerable variations. The following are the readings :—

		Temperature of rectum.		Remarks.
12 noon	...	37·3	...	
12.5	...	39·1	...	
12.10	...	38·9	...	
12.12	...	38·8	...	·13c.c. of $Ni(CO)_4$ injected under skin of back.
12.13	...	38·5	...	
12.14	...	38·5	...	
12.16	...	38·4	...	
12.17	...	38·4	...	
12.18	...	38·35	...	
12.20	...	38·2	...	
12.21	...	38·2	...	
12.22	...	38·1	...	
12.23	...	38·0	...	No convulsions. Respiration slower.
12.24	...	37·85	...	
12.25	...	37·7	...	
12.26	...	37·68	...	
12.27	...	37·6	...	
12.28	...	37·5	...	
12.28.5	...	—	...	Twitching of right hind leg.
12.29.5	...	37·4	...	
12.30	...	37·35	...	
12.31	...	37·28	...	
12.32	...	37·19	...	Twitching of right hind leg.
12.33	...	37·1	...	
12.34	...	37·0	...	
12.35	...	36·91	...	
12.36	...	36·81	...	
12.37	...	36·8	...	Slight twitching.
12.38	...	36·8	...	
12.39	...	36·7	...	

	Temperature of rectum.	Remarks.
12.40 ...	36·6 ...	
12.41 ...	36·5 ...	
12.42 ...	36·47 ...	
12.43 ...	36·39 ...	
12.44 ...	36·31 ...	
12.45 ...	36·25 ...	
12.46 ...	36·18 ...	
12.47 ...	36·1 ...	
12.48 ...	36·01 ...	
12.49 ...	35·94 ...	
12.50 ...	35·95 ...	Thermometer pushed slightly farther in.
12.51 ...	35·95 ...	
12.52 ...	35·9 ...	
12.53 ...	35·89 ...	Micturates on instrument.
12.54 ...	— ...	
12.55 ...	— ...	
12.55.5 ...	35·72 ...	
12.56 ...	35·7 ...	
12.57 ...	35·51 ...	
12.58 ...	35·49 ...	Twitching of right hind leg resumed.
12.59 ...	35·4 ...	
1.0 p.m. ...	35·38 ...	
1.1 ...	35·3 ...	
1.2 ...	35·27 ...	
1.3 ...	35·26 ...	
1.4 ...	35·2 ...	
1.5 ...	35·11 ...	Marked twitching.
1.6 ...	35·09 ...	
1.7 ...	35·06 ...	
1.8 ...	35·02 ...	
1.9 ...	35·99 ...	
1.10 ...	34·91 ...	
1.11 ...	34·9 ...	
1.12 ...	34·88 ...	
1.13 ...	34·9 ...	Micturates on thermometer.
1.14 ...	34·9 ...	
1.15 ...	34·82 ...	
1.16 ...	34·72 ...	Animal allowed to run about a little. Shows weakness of hind limbs. Vessels in ear full of bright red blood.
1.29 ...	33·3 ...	Animal lies quiet on table without being held. Slight tremor in right hind leg. Twitching of right upper eyelid.
1.34 ...	32·5 ...	
1.47 ...	31·93 ...	

	Temperature of rectum.	Remarks.
1.51 ...	31·6 ...	
1.55 ...	31·4 ...	
2.0 ...	31·0 ...	
2.5 ...	30·47 ...	
2.12 ...	30·0 ...	
2.15 ...	29·5 ...	
2.17 ...	— ...	Severe convulsive seizure. Gasping
2.20 ...	29·3 ...	
2.25 ...	28·6 ...	
2.34 ...	28·1 ...	Rabbit placed in a large jar of pure oxygen.
2.47 ...	26·3 ...	
3.0 ...	26 ...	
3.5 ...	— ...	Respirations 52 per minute.
3.13 ...	— ...	Respirations 40 per minute. Taken out of jar of oxygen, and placed in a box before the fire, to warm the body, which had become perceptibly cold. The temperature rose up to 28·5.
4.30 ...	— ...	Animal died after a severe convulsive spasm.

This experiment shows that, after a subcutaneous injection of ·13c.c. of $Ni(CO)_4$, the temperature steadily fell from 38·8° C. to 26° C., and that the animal died in 4 hours 18 minutes. There was a fall of temperature of 12·8°.

Experiment 7.—A rabbit weighing 572 grms. was wrapped in a towel, and the temperature of the rectum was taken at intervals.

	Temperature of rectum.	Remarks.
12.10 ...	38·9 ...	Thermometer in rectum.
2.15 ...	37·0 ...	
2.20 ...	37·0 ...	
2.25 ...	— ...	Thermometer accidentally came out.
2.30 ...	36·5 ...	
2.35 ...	35·8 ...	
2.36 ...	35·5 ...	Thermometer removed.
2.38 ...	35·6 ...	
2.45 ...	35·6 ...	
2.50 ...	36 ...	
2.55 ...	35·9 ...	Animal released and allowed to run about.
3.5 to 3.20	36·6 ...	Temperature remained fairly constant.

As already stated, we found that a slight variation in the distance to which the thermometer was pushed into the rectum

caused a considerable variation in the reading, and, to secure accuracy, we had a mark on the thermometer, and the instrument was inserted up to this mark in each observation.

		Temperature of rectum.		Remarks.
3.21	...	—	...	·06c.c. injected below skin of back.
3.27	...	—	...	Rabbit less lively; slight paralysis of hind limbs.
3.30	...	35·8	...	Vessels in ear full of bright red blood.
3.35	...	—	...	Fore limbs weak; animal resting on belly.
3.40	...	34·7	...	
3.45	...	—	...	Marked weakness of right leg.
3.47	...	—	...	Both legs extended.
3.50	...	34·3	...	
4.4	...	34·2	...	Head falling forwards.
4.10	...	33·2	...	No attempt to pull itself together when the animal's limbs were moved.
4.20	...	33·7	...	As animal's feet cold, it was placed before the fire.
4.30	...	33·9	...	After gasping respiration, animal died.

In this case ·06c.c. killed the animal in 1 hour 10 minutes, and the temperature fell from 36·6° to 33·9°, a fall of 2·7°. The seat of the injection was somewhat congested. Chemical tests showed the presence of nickel in the tissue of that region. The blood also gave reactions showing the presence of nickel.

As ·06c.c. of the pure liquid $Ni(CO)_4$ represented 1 minim of the fluid, and as we were desirous of giving smaller doses, we next attempted to use a solution of 1 of $Ni(CO)_4$ in 10 of chloroform. This we termed the $\frac{1}{10}$th solution, and 1 minim represented ·006c.c. of pure $Ni(CO)_4$.

The following experiments were performed with this solution :—

Experiment 8.—Rabbit weighing 720 grms. Respirations 54 per minute. Temperature of rectum taken repeatedly at intervals, and found to be fairly constant at 38·3°.

		Temperature of rectum.		Remarks.
3.47	...	—	...	1 minim (·006c.c. of $Ni(CO)_4$) of nico-chloroform solution injected under skin of back.
3.55	...	37·1	...	
4.5	...	36·2	...	
4.10	...	—	...	Respirations 48 per minute.
4.13	...	—	...	Animal eating.

		Temperature of rectum.		Remarks.
4.15	...	35·6	...	
4.20	...	—	...	Eating freely.
4.25	...	35·75	...	
4.35	...	35·7	..	
4.45	..	36·2	...	
4.55	...	36·3	...	
5.5	...	36·5	...	
5.15	...	36·5	...	
5.25	...	36·0	...	
5.35	...	35·9	...	Animal left running about in a warm room.
7.0	...	36·4	...	
8.30	...	37·0	...	Animal seemed vigorous and uninjured.

Experiment 9.—On the following day, at 10.35 a.m., the same rabbit had a temperature of 37·35°.

		Temperature of rectum.		Remarks.
10.40	...	—	...	1 minim (·006c.c. of $Ni(CO)_4$) of nico-chloroform injected under skin of back. (The fluid showed a slight turbidity of a greenish colour.)
10.50	...	36·5	...	
11.0	...	36·35	...	
11.10	...	36·2	...	
11.20	...	35·9	...	
11.30	...	36·0	...	
11.40	...	36·0	...	
11.50	...	35·9	...	
12.0	...	36·0	...	
12.10	...	35·6	...	
12.20	...	35·8	..	
12.30	...	35·7	...	
12.50	...	35·7	...	
4.40	...	36·7	...	

The animal throughout the experiment appeared to be little affected, but there was a fall of temperature. In both experiments, 8 and 9, the temperature rose after the effect of the injection had passed off.

Experiment 10.—The same rabbit as was subjected to Experiments 8 and 9 was again used. At 1.35 the temperature of the rectum was 38°, and it was found to be fairly constant. The solution of $Ni(CO)_4$ in chloroform showed a dirty greenish precipitate,

which was suggested to be hydrated oxide of nickel, produced by the chloroform not being anhydrous.

		Temperature of rectum.		Remarks.
1.36	...	—	...	2 minims (·012c.c. of $Ni(CO)_4$) of nico-chloroform were injected under the skin of the back.
1.45	...	37·9	...	
2.0	...	38·0	...	
2.5	...	—	...	Animal drowsy, as if affected by chloroform.
2.20	...	37·2	...	
2.50	...	37·0	...	
3.15	...	37·0	...	
4.10	...	36·8	...	
4.35	...	36·5	...	

In this experiment there was a slight fall of temperature. The rabbit, although carefully looked after, was found dead in its hutch two days later. The bladder was distended with urine, the kidneys were congested, there was serous fluid in the pricardium, the left ventricle was empty and the right was full of clot. No trace of nickel could be discovered in the heart, blood, or urine. Probably the chloroform solution had decomposed, and no $Ni(CO)_4$ was present. The slight fall of temperature may have been due to the chloroform.

We attempted, but without success, to use a solution of $Ni(CO)_4$ in pure olive oil. Thus: the point of a small sealed tube containing ·75c.c. of $Ni(CO)_4$ was broken underneath the surface of 86c.c. of pure olive oil.

Experiment 11.—A rabbit, weighing 1290 grms., at 2.46 had a temperature in the rectum of 38·1° C.; at 2.49, 20 minims of olive-oil-solution of "nico" (1/85c.c. of $Ni(CO)_4$) were injected under the skin of the back. At 3 o'clock, there was no perceptible effect; at 3.1, the temperature was 38°·1 C.; at 3.24, 37°·7 C.; at 3.43, 37°·6 C.; and at 4, 37·6 C.

Experiment 12. — A small house-mouse had 10 minims = 1/172c.c. of $Ni(CO)_4$ of olive-oil-solution of "nico" injected under skin of back. No effect was observed.

We next made a solution of $Ni(CO)_4$ in pure anhydrous chloroform, as follows:—A small sealed tube, containing ·75c.c. of $Ni(CO)_4$, was broken in a dry, well-stoppered bottle containing 16c.c. of anhydrous chloroform, and 1 minim represented 1/300c.c. of $Ni(CO)_4$.

Experiment 13.—A rabbit weighing 1460 grms. was the subject of the next experiment.

	Temperature of rectum.	Remarks.
2.34	—	Injected 10 minims of chloroform-solution of "nico." 1/30th c.c. of $Ni(CO)_4$.
2.36	39·0	
2.50	38·6	Weakness of hind limbs.
2.57	38·5	
3.0	38·3	
3.5	38·25	
3.10	38·2	
3.15	38·0	
3.20	37·9	
3.25	37·7	
3.30	37·5	
3.35	37·2	
3.40	37·0	
3.45	36·8	
3.50	36·6	
3.57	36·3	
4.0	36·2	
4.7	36·1	Animal eating turnip.
4.12	35·8	
4.15	35·5	Animal left with complete paralysis of hind limbs, and the temperature still falling.
6.45	33.2	Hind legs still paralysed; not eating; pupils dilated.
7.35	32·8	

Next day the animal was alive, and the hind limbs were paralysed.

	Temperature of rectum.	Remarks.
11.15	29·5	In rectum.
	31·3	In mouth.
12.30	32·2	Mouth.
3.30	32·5	Mouth.
5.30	32·5	The animal was kept in a box near the fire.
6.20	33·5	
8.30	34·7	
9.40	34·4	Still paralysis of hind limbs.

At 10.40 on the following day the temperature of the rectum was 31·3° C. The animal was weak, refused food, and the bowels were loose. Next morning it was found dead.

This experiment shows that the 1/30th c.c. (·033) of $Ni(CO)_4$ in ·4c.c. of anhydrous chloroform was lethal, and caused a fall of temperature of no less than 9·5°.

As it is well known that chloroform causes a fall of temperature a control experiment was performed in which the same quantity of anhydrous chloroform was given as in Experiment 13, but without $Ni(CO)_4$.

Experiment 14.—A rabbit, weighing 1342 grms., had at 4 p.m. 10 minims (·6c.c.) of anhydrous chloroform introduced below the skin of the back.

	Temperature of mouth.	Remarks.
4.0	38·8	
4.25	38·5	
4.30	—	Rabbit drowsy.
4.35	37·6	
5.0	37·3	
5.15	36·9	
5.30	36·1	
5.45	35·5	
6.0	34·7	
6.15	34·3	
6.25	34·1	
8.30	35·2	Rabbit running about.
9.17	36·0	
9.35	36·0	

At 10.45 a.m. next day the temperature was 38·2°, and the animal was quite well. This experiment showed a fall of temperature 4·7° in the course of 2 hours 25 minutes, when the temperature began to rise, and the normal temperature was regained next morning. In Experiment 13, the temperature had fallen 5° in about the same period of 2 hours 25 minutes, but it continued to fall for 24 hours longer, when it began slowly to rise. The effect of $Ni(CO)_4$ was therefore much greater and more persistent than the effect of the chloroform in which it was dissolved.

As 1c.c. of the liquid $Ni(CO)_4$ at 17° C. contains ·4470 grm. of nickel, we next studied the action of a corresponding amount of nickel in an aqueous solution of nitrate of nickel of such a strength that 1c.c. contained ·4470 grms. of nickel. Two solutions of this in water of the strength respectively of 1 in 300 and of 1 in 20 (the same strength as in Experiment 13) were used.

Experiment 15.—A rabbit weighing 1836 grms. had, at 4.30, a temperature in the mouth of 38·9. Ten minims (·6c.c.) of

1/300th solution of nitrate of nickel in the water containing the same amount of nickel as 1/450th c.c. of "nico" were injected under skin of back. The temperature was unaffected. At 6.16 it stood at 39·1 and at 8.45 at 38·6. Then 10 minims (·6c.c.) of 1·20 solution containing the same amount of nickel as 1/30c.c. of "nico" (as in Experiment 13) were injected below skin of back. At 9.15 the temperature was 38·9, at 9.40 at 37·6, and at 10.50 on the following day it was 38·8. We may conclude that a reduction of temperature is not caused by a quantity of nickel equal to that in the doses of $Ni(CO)_4$ which produced the remarkably persistent fall of temperature, as in Experiments 6 and 8.

We next endeavoured to examine roughly the gases of the blood after poisoning by $Ni(CO)_4$.

Experiment 16.—A rabbit, weighing 997 grms., at 2·58 had 2 minims (·13c.c.) of pure $Ni(CO)_4$ injected below skin of the back.

		Temperature of mouth.	Remarks.
2.58	...	38·7	
3.13	...	37·6	
3.21	...	37·5	
3.30	...	36·3	
3.40	...	36·1	
3.50	...	35·1	
4.4	...	33·1	
4.47	...	—	Animal died after severe convulsions.

The animal was bled from the neck, and the blood was quickly introduced into a Torricellian vacuum produced by a mercurial gas pump. A small quantity of gas was obtained, and this was found on analysis to contain carbonic oxide.* No trace of nickel was found in the gases, but it was found in the blood from which the gases were drawn.

Two experiments were made in which the animal was caused to breathe an atmosphere containing the vapour of $Ni(CO)_4$.

Experiment 17.—A rabbit, weighing 1090 grms. was placed in a belljar containing 24 litres of air; a small tube containing ·6c.c. of $Ni(CO)_4$ was broken inside the jar and the fluid evaporated very quickly. As 1c.c. of the liquid, $Ni(CO)_4$ at 17° C., contains ·87146 grm. of CO, which at 0° and 760 mm. yields 696·5c.c. of gas, 0·6c.c. would yield about 417·9c.c. of CO gas, as 1 molecule of

* The gas analysis was performed by Dr. G. G. Henderson, Chemical Laboratory, University of Glasgow.

$Ni(CO)_4$ contains 4 molecules of CO, the animal breathed an atmosphere containing 0·432 per cent. of $Ni(CO)_4$ gas.

3.0	... ...	Animal placed in jar.
3.7	... ...	Agitated, muscular tremors, urination, defæcation.
3.13	... ...	Animal sitting up on hind legs; no paresis; vessels of ear full of bright red blood.
3.25	... ...	Weak in movements, but no marked symptoms; removed from jar.
3.26	... ...	Temperature of mouth 37·3.
3.40	... ...	Trembling and weak on legs.

The rabbit was found dead next morning, showing a lethal effect after breathing an atmosphere containing ·432 per cent. for 25 minutes.

Experiment 18.—A rabbit, weighing 489 grms., was placed at 3.45 in the 24 litre jar, the air of which contained ·58c.c. of $Ni(CO)_4$ = 0·41 per cent.

3.45	... ...	Animal placed in jar.
3.46	... ...	Vessels in ears full of bright red blood; animal agitated.
3.56	... ...	No very evident effect, except animal rubbing its nose with fore paws.
3.57	... ...	Another quantity of ·69c.c. of $Ni(CO)_4$ = 0·49 per cent. of $Ni(CO)_4$ gas was added. The air contained now nearly 0·9 per cent. of $Ni(CO)_4$.
4.0	... ...	Violent convulsions; animal lying on belly with the hind legs stretched out. Great difficulty in breathing. The animal was quickly removed and placed in a large jar of pure oxygen.
4.10	... ...	Died in severe convulsions.

It was rapidly bled, and the blood was passed into vacuum, and a small quantity of gas was obtained. This gas contained carbonic oxide, but no trace of nickel. The metal was found in the blood. Quantitative measurements of the gases were not made, as the object, at this stage of the inquiry, was to ascertain the presence or absence of carbonic oxide in the blood.

The general results of the investigation may be briefly summarised thus:—

1. $Ni(CO)_4$, is a powerful poison when injected subcutaneously.
2. The vapour of $Ni(CO)_4$ in air, even to the extent of less than 0·5 per cent., is dangerous.

3. The symptoms are similar to those caused by carbonic oxide.
4. The spectrum of the blood of an animal poisoned by $Ni(CO)_4$ is that of carbonic-oxide-hæmoglobin, and it is not reduced by sulphide of ammonium.
5. When the substance is injected subcutaneously it is probably in part dissociated in the tissues, as there is evidence of the existence of nickel in those tissues, but the nickel also finds its way into the blood and is found there.
6. The substance produces a remarkably prolonged fall of temperature even when given in small quantities. This may be accounted for by the hæmoglobin being prevented, to a large extent, from supplying the tissues with oxygen. "Nico," as we may for convenience term this remarkable substance, makes it possible to give graduated doses of carbonic oxide and thus to reduce temperature by directly interfering with the respiratory exchanges occurring in the tissues. The objections to its use as an antipyretic are that, owing to its poisonous properties, it is difficult to inject it subcutaneously in sufficiently small doses, while it is not easy to obtain a solution in any menstruum in which decomposition will not take place. If a convenient method of dissolving it could be devised, $Ni(CO)_4$ might become a valuable antipyretic, the *modus operandi* of which is intelligible.

We intend to continue the investigation more especially as to (*a*) the solubility of the substance; (*b*) the quantitative effects on the gases of the blood; and (*c*) the quantitative effects on the gases of the air breathed by an animal under its influence.

XIII.—*The Progress of Sanitation, with Special Reference to the Sanitary Condition of our Glasgow Public Schools.* By W. P. BUCHAN, Sanitary Engineer, President of the Sanitary and Social Economy Section of the Society.

[Summary of Paper read before the Society, 18th February, 1891.]

MR. BUCHAN briefly alluded to the attention given to the subject of sanitation from the earliest times, and referred to the ravages made by some of the great plagues and epidemics of the middle ages,* more especially to that which swept over England in 1665. Passing on to what may be called modern sanitation, he spoke of the labours of Dr. Southwood Smith and Mr. Edwin Chadwick in the cause of sanitary reform. Progress had been slow, but an impulse had from time to time been given to it by great public calamities, such as cholera, the great loss of life through disease during the Crimean war, the death of the Prince Consort, and the threatened loss of the Prince of Wales. The author then proceeded to say:—In the Crimean war, 1854-55, the loss of the soldiers in actual battle was small in comparison with the large numbers carried off by sickness, largely resulting from insanitary conditions. Of the 22,000 men lost by the British army, only 4,000 died in battle, or of wounds—18,000, or more than four times as many, dying of disease. The French army, however, lost about 100,000 men—20,000 of these in battle, and 80,000 by disease. The Russians suffered still more, as they lost about 80,000 in battle, and about 500,000 from disease.

In the American Civil War the Northern army lost 97,000 men by battle, and 184,000 from disease; while the Southern army lost about half-a-million men—the greater number of these from disease. All this immense loss from disease is truly appalling.

As showing the beneficent power of sanitation the author referred to what occurred when the news of the disastrous condition of the British army in the Crimea reached this country. In the spring of 1855 Mr. Robert Rawlinson, C.E., was sent out as engineering sanitary commissioner—Drs. John Sutherland and Hector Gavin being the medical commissioners. On their arrival at Balaclava

* At this period science was scorned, but Nature, that has no respect for faith based upon ignorance, took a terrible revenge.

on 3rd April, the mortality was as high as 9·61 per cent. per month. By the end of June—or in three months' time—it was reduced to 1·01 per cent., or nearly ten times less. Previous to the appearance of the sanitary commissioners on the scene, the losses of some regiments rose as high as 70 per cent. It was at this time that the Government sent out a number of trained female nurses, headed by Miss Nightingale, to tend the sick and wounded soldiers, and their advent proved a great blessing.

The lesson taught in the Crimea was not lost, for while the loss of soldiers by death in the United Kingdom was 18 per 1,000 yearly before the Crimean war, in 1878 it was only about one-third of that, or 6·53 per 1,000.

Prisons, again, which were at one time pest-houses, are now among the healthiest places in the country.

As sanitation rests upon a physical basis, when the proper means are taken, sanitary progress follows as a natural consequence. As a proof of this we have only to look upon the history of our own city previous to the inauguration of the Loch Katrine water supply, and compare that with our condition and death-rate of to-day. Cholera and typhus fever have killed or permanently injured thousands of our citizens. They raised the death-rate in 1832 to fully 46 per 1,000. In 1837, with an epidemic of typhus, the death-rate was 40·6 per 1,000; but ten years after, from destitution and insanitary conditions, the death-rate rose much higher still—namely, to 52·5 per 1,000. In 1848-49 the city had a second visitation of cholera, the probable number of cases* being about 8,000, and the deaths about half that number. In 1853-54 there was a third visitation of cholera, the death-rate in the latter year being 42·4 per 1,000—the deaths from cholera being 3,885.

In October, 1859, Her Majesty the Queen turned on the water of Loch Katrine to the city, and when, about seven years after—in 1866—cholera invaded the city for the fourth time, Glasgow was so improved in its sanitary conditions that the deaths were only 53. Three years after this, however—namely, in 1869,—there was an epidemic of typhus fever giving 970 deaths, which raised the death-rate to 34·09 per 1,000.

In 1872 our Past-President, Dr. J. B. Russell, was appointed Medical Officer of Health for the City, and since then the death-

* See Dr. Glaister's paper, read before the Society on April 14th, 1886.

rate has only once reached 30 per 1,000—namely, in 1874, when, owing to an epidemic of scarlatina which caused 1,719 deaths, the death-rate rose to 31·4 per 1,000. It was about this time that Dr. Russell drew attention to the great danger arising from the contamination of the milk supply. In 1875-77 and in 1880 there were epidemics of enteric fever in the city, which Dr. Russell in each case traced to tainted milk brought from infected farms. For his work in this relation he deserves the sincerest thanks of the whole community. The average death-rate of the four years ending with 1889 was down to 23 per 1,000. For this great reduction in the death-rate—and long stride in the right direction—our thanks are largely due to the work of the Sanitary Department, and to the wise action of the City Councillors in rooting out a large number of crowded, old, and unhealthy buildings.

My remarks would be incomplete in this connection without going back a little to refer to the sanitary work of one of our former Presidents, whose experiments and investigations did so much to establish the view that sewer gases entering dwelling-houses produced disease, and that they often found an inlet there through their power of corroding and perforating lead pipes. I refer to the late Dr. Andrew Fergus.*

Following upon this, a great practical advance was made when improved systems of disconnecting the house drains from the public sewers were introduced, and also of properly trapping and ventilating said drains and the soil and waste pipes leading into them, and, further, when the system was invented of testing drains and soil pipes to detect leaks by means of smoke. The smoke test is now an established and recognised factor in sanitary work, acknowledged in our laws, and in constant use, while suitable mechanical contrivances for applying it have rendered it very easy and effective.†

* He died 30th July, 1887. See his Memoir by Dr. Duncan, p. 245 of the Society's *Proceedings*, Vol. XIX.

† The first time I remember of applying the smoke test in a drain was at the end of April, 1875, at 128 Renfield Street, Glasgow, when the old buildings were there. The first Buchan's intercepting drain trap—made hurriedly of metal—was put in there, and on its house side some smoke-producing stuff was burnt, so that the smoke went into and along the drain (which was under the floor of the house) and up the soil pipe, and out at the top of its air pipe above the middle of the roof. Leakage in the drain was indicated by the smoke appearing inside of the house. The smoke-machine testing for the drains was introduced by me shortly after this.

If sanitation is to progress we must continue to use the proper physical means for that purpose. Some of you will remember that when Lord Palmerston was asked to proclaim a national fast-day on account of a visitation of cholera, he declined to do so, telling the people that the fault lay with themselves, and that they were to set to work to whitewash their houses, and clear away the filth that lay about them. Charles Kingsley said:—"As a clergyman, I feel bound to express my gratitude to Lord Palmerston for having refused to allow a national fast-day on the occasion of the present reappearance of pestilence, and so having prevented fresh scandal to Christianity, fresh excuses for the selfishness, laziness, and ignorance, which produce pestilence, fresh turning men's minds away from the real causes of this present judgment to fanciful and superstitious ones. It was to be hoped that after the late discoveries of sanitary science, the clergy of all denominations would have felt it a sacred duty to go forth on a crusade against filth, and so to save the lives of thousands, not merely during the presence of cholera, but every year."

While the carrying-out of house drainage may be said to have now risen to the dignity of a science, there is still much to do before a satisfactory solution of the disposal of the sewage problem has been reached. In my opinion the East-End purification experiments should not have been undertaken, but now that they have been decided upon I think they ought to get a fair trial; while, to enable a proper judgment to be got of the condition of the effluent, I think the weir ought to be rebuilt. This would not only enable that to be better done, but also give better facilities for boating. The state of the river at the Green for years back has been very bad.

While great strides have been made in sanitary progress in many departments, there is one in which we have lagged behind very much. I allude to the ventilation of our buildings. Whether it be houses, churches, halls, or schools, we are still far from being up to the mark as regards this. Few houses have any systematic means for ventilation provided, while the condition of many churches must be very disagreeable to the worshippers, and in many cases not very creditable to them. Then, as to halls, few of them are properly ventilated. Our own City Hall has been again and again complained of in the public papers lately.

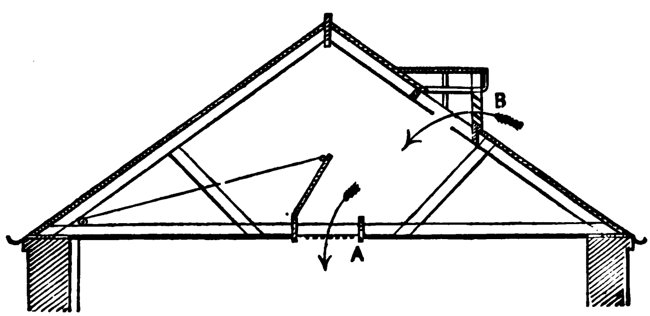
But I wish to speak more particularly to-night of the ventilation of our public schools. On 16th November last Sir John Neilson

Cuthbertson was chairman at a meeting held at the opening of the new Board School in West Street, Calton, and in presence of the Right Honourable the Lord-Advocate, who was also there, he said :—" In Glasgow they had had no experience of the shoddy work which was alleged to have been passed off on other Boards. After seventeen years he could say that not one of the schools erected had proved anything but satisfactory."

Now, there is no doubt that, looking at many of the new Glasgow schools from the outside, they are palatial buildings, but I am not sure that they could stand a searching scrutiny in every particular.

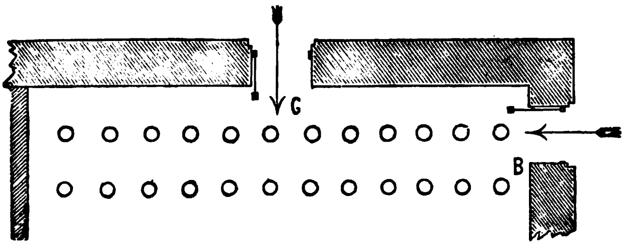
I was anxious recently to publish something commendatory as well as descriptive of the schools of Glasgow, especially in regard to their ventilation. For this purpose I visited a number of them. Some of these are under the management of the Glasgow School Board and some are not. I have to thank the teachers of the schools for their courtesy on the occasion of my visits. A few of them were satisfied with the condition of the air in their school-rooms, but the great majority complained strongly of the imperfect ventilation.

In regard to the first one visited I found on more than one occasion in the afternoon that the air was close and disagreeable, and I learned that unpleasant results were experienced from down-draughts when the wind blew against the face of the ventilator. That it could not be otherwise will at once be evident from the diagram, where A, in the accompanying figure, shows the intended outlet in the ceiling, and B the " ventilator " on the roof.



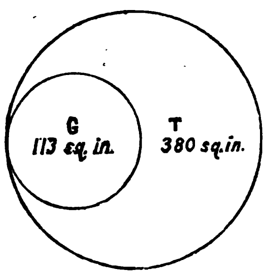
Taking another school, which I understand was erected by the Board nine years ago, the provision for its ventilation when I visited it in September, 1890, was, in my opinion, unsatisfactory. The

rooms on the ground floor had no pipes for carrying off the vitiated air from the ceilings, while the fresh air was admitted by gratings and windows, which latter hinged to open inwards, as shown at B and G in the diagram. The upstairs rooms, while having their



windows also hung in casement fashion, to open inwardly, have each a pipe off the ceiling, although said pipe is much too small, and especially for the larger room, which has only a 12-inch diameter outlet-pipe (equal to 113 square inches) for 110 children. This gives only about 1 square inch of outlet for each pupil.

Contrast with this a new school at Galashiels, which has an outlet-pipe equal to about $3\frac{1}{2}$ square inches for each pupil, and with provision against down-draughts. At another school in Haddingtonshire the largest room, intended for 110 pupils, has an outlet-pipe 21 inches in diameter, or fully three times the area of that in the Glasgow school referred to. The subjoined diagram will give an idea of the relative sizes of the pipes, the *smaller* circle indicating the Glasgow allowance!

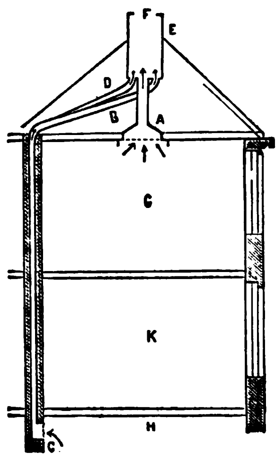


With great care and attention the teachers may do a little towards preventing some of the evils liable to ensue from this state of matters, but it is neither fair nor safe to lay this extra burden upon their shoulders. Health is the only capital many persons possess, hence the greater sin to cause them to lose it.

Another Board school that I visited had its provision for letting in fresh air on a much improved style in comparison with the last. The incoming air takes an upward direction, and can be heated. The provision for carrying off the vitiated air from the ceiling is very much too small, however, being only about $1\frac{1}{4}$ square inches for each pupil. A newer public school, one opened

last summer, was afterwards visited by me. The air in most of the rooms there was not what it should be, the provision for carrying off the vitiated air being both too meagre and badly arranged, so that in some of the rooms the windows were open from the top a considerable distance, allowing, as I considered, the cold winter air to blow down upon the children in a way to to be very uncomfortable and dangerous for them. When I shut the windows and tested the speed of the air up the outlet-pipes, which at the top are about 9 inches in diameter, it was very sluggish : for example, on October 17th, 1890, in the ground flat, north-east room, with three windows 5 inches down from the top, and which had two outlets in the room, the speed of the outgoing air was only 80 linear feet per minute. In the south-east room, one stair up, which had 84 pupils, and in which three of the windows were 9 inches open, the outlet speed at 18 inches by 12 inches outlet was only 70 linear feet per minute, but at the other outlet, 9 inches by 9 inches, it was 115 linear feet per minute. At the rate at which the air was going out from one of the rooms I calculated that it would take about an hour and a half to change the air—or rather to empty a roomful of it—once.

In three of the large new Board schools, opened in Glasgow last year, the style in which the provision for carrying off the vitiated air is carried out, is, in my opinion, not only very bad, but also far too meagre. A wind-acting exhaust ventilator is set up above the roof, and is made to do service for several rooms on different flats, and in some cases, as indicated in the sketch here shown, the largest branch pipe into the main pipe of the ventilator is from the room in the top flat near it; while the pipes from the rooms farther away are smaller and have one or more bends in them, so that often, and especially when there is little wind, the ventilator may get almost its whole supply from the larger pipe of the room in the top flat near it (especially if its windows are open), while scarcely any current is coming up from the rooms below. This happens all the more because the vertical shafts from the lower rooms (K and H) are rough



passages in the brick walls, while the shaft (A) from off the ceiling of the top-flat room (G) is a smooth galvanised iron or sheet zinc pipe.

When I experimented in Washington Street new School the outlet in a top-flat room indicated about 450 lineal feet per minute, but when I went down to one of the lower rooms, I had great difficulty to get an indication of as much as 40 feet per minute. I pointed out this most unsatisfactory state of matters lately to the School Board officials.

I also examined the new schools in Napiershall Street and the one in West Street, Calton. The provision for carrying off the vitiated air from both is much too little, and in a number of the rooms is done on the principle I have just sketched, and which, in my opinion, is very defective. The style of the fresh air inlets is also open to improvement.

It is likewise right to point out that in a number of cases the intended wind-acting exhaust ventilators are actually set up *below* the ridge, so that when they are on the lee side of the roof the wind cannot get at them to help them to work, while, when they are on the windward side, the tendency is for them to allow down-draughts very readily.

I have been speaking principally about the schools under the Glasgow School Board and some not so, all which are nominally ventilated by natural or automatic means; but there are a few schools in Glasgow, however, not under the Glasgow School Board, which I ought to refer to before closing, as their ventilation in whole or in part is much better than those I have been describing.

In one case the ventilation is aided or carried out by artificial or mechanical means. In my opinion this is the best school I saw in Glasgow. It is ventilated by means of a 2-horse power Otto gas-engine, driving a 48-inch diameter Aland's fan, placed a few feet below the ridge of the roof. The fan extracts the air from the various rooms. The main outlet at the ridge of the roof is 6 feet 4 inches long by 2 feet 6 inches wide, and, when I tested it, the air was going out at a speed of fully 1,000 lineal feet, or 16,000 cubic feet, per minute—the air being changed in the rooms about four times an hour, which is six times oftener than that of the Glasgow Board school-room, automatically ventilated, which I said required about an hour and a half to change the air once.

The cost per pupil for a whole year of the expense of keeping up this fan and engine at this three-flatted school (of, say, 1,000

pupils) is only about fourpence each. The school being at Hillhead is under the Govan Board.

A country school of only one flat may be well ventilated automatically quite easily, but this is not the case with large three-flatted schools, like many of those in Glasgow. The ventilation of the new part of the Allan Glen Technical School is effected by means of a large chimney stalk into which shafts or flues from the various rooms are joined. The chemical class-room has two flues along its ceiling leading into the chimney, one fully 30 feet long, and the other, square in section, about 80 feet long. Each flue has a number of inlets into it. The results of tests which I made with these have been rather interesting. For example, the speed at which the air passed into the end opening of the 30-feet flue was 700 lineal feet per minute, but the speed at the far end of the 80-feet flue was only 30 feet per minute; at the third opening from the far end, 140 feet; at seventh opening, 370 feet; at ninth, 570 feet, the nearer the chimney, the quicker. This supports my condemnation of the joining of pipes from the lower flats of a school into the same wind-acting ventilator that serves for the top-flat room, and especially when the pipes farthest away are the smallest.

In a South-Side school, lately ventilated by motive power, when I examined it, the air from a large room near the fan was going out at the rate of 1,200 lineal feet per minute, which kept the room air fairly pure, but when I entered a larger room which had the same size of piping from it, the air was very impure. The reason was, that not only were there more pupils in this room than in the other, but the air, owing to the room being much farther from the fan, was only going out at 600 lineal feet per minute, or half the rate in the smaller room near the fan. In this case the piping, instead of being only about 9 inches diameter, should have been, at least, 14 inches diameter for the larger room farthest from the fan.

With regard to the character of the air in schools, Professor Carnelley wrote me, a few months before his death, to say that he intended to test the air in the Glasgow schools, but he was suddenly cut off before he could accomplish it, and so a brilliant career was closed, to the great loss of sanitary science.

In conclusion, I have to say that this subject of improved ventilation must be kept before our School Boards. No doubt they have been animated by a desire to do the best they could,

according to their lights, for the health and comfort of the children entrusted to their charge, and it is with regret that I have found it necessary to point out serious defects in their buildings, especially in this matter of ventilation. It is to be hoped, however, that as more attention has been given to this matter of late, and improved ways of doing the work are being introduced, that our own School Board will yet look carefully into this highly important subject, and for the sake of all concerned, do what is necessary to improve the atmosphere of the schools. When that is done I shall have much pleasure, in some future address or publication, to give expression to my heartiest commendations.

XIV.—*An Inquiry into the Nature of Heredity.* By WILLIAM WALLACE M.A., M.B., C.M., L.D.S.

[Read before the Society, 1st April, 1891.]

THE theory of the succession and reproduction of forms of life here submitted was rendered in similar terms before the Medico-Chirurgical Society of the students of the Glasgow University, about five years ago. In that, as well as in this, paper, the idea of heredity or hereditary transmission is regarded as a psychological error. Within comparatively recent years an appeal to histological facts has occasioned a fundamental change in the interpretation of this biological principle. Up till then, not only had it gained almost universal assent, but it had also been looked on as an idea easily understood and easily illustrated; now it has become a problem of the greatest complexity. The theory, already gaining very general assent, of the continuity of reproductive substance, a deduction of the truth *omnis cellula e cellulâ* forms an easy transition from the older notions to the opinion expressed in this paper. The historico-critical commentary on the different theories of heredity, delivered here a few sessions ago by Dr. M'Kendrick, shows the nature of this recent change. Amongst the writers then reviewed, there is so much *inter se* contradiction that even while the latest of them insist on the necessity of being guided by the morphological facts which pertain to the arguments, there is reason to believe that there is still a fallacy dominating and confounding them. They seem indeed to be negligent of the necessity of formal reasoning. There are already many who find it impossible to believe, and regard it as inconceivable, that mutilations, diseases, acquired characters, the effects of use and disuse, and habit, and the influence of the environment, can be the subjects of inheritance. Indeed, all the sources from which heredity used to draw her supplies have been cut off. It is now said that these sources were never in contribution. The very phenomena which gave birth to the idea of heredity, and which heredity was supposed to account for, cannot now be correlated with heredity. The word has been

retained and correlated with the phenomenon of the continuity of germ plasm. It may be shown, however, that it cannot be applied to this phenomenon either, consequently it may be set aside as a word without biological application in a scientific sense.

An analogy may be drawn between the development of the body and the development of the mind. The body, equally with the mind, may be regarded either as the outcome of an innate tendency, or as the product of external causes. So certain psychologists incline towards the idea that mental development is the result of innate causes; and there are biologists—indeed a plethora of them—who attribute organic development to heredity or inherent tendencies. This principle brings parent and offspring into the relationship of cause and effect. Atavism brings the pre-parental generation and offspring into the same relationship. Other psychologists think that the mind of itself has no innate tendency to develop, and they rather attribute its development to the agency of experience and external circumstances. These thinkers have in their digressions discredited and doubted the idea of heredity. If we believe in an hereditary tendency of the body or an innate tendency of the mind to develop, we believe that the cause of development exists before development takes place; but if we believe that the causes of each change in the development of the mind or body operates in connection with that change, then the development of the individual is due to causes operating in its lifetime. The latter seems to be a necessary belief. It is impossible to conceive a developmental change apart from the forces or agencies that bring it about, and the energy that is expended in any morphological change must be expended at the time of the change. It might, therefore, be contended on deductive grounds that the organic and mental development of each individual is the resultant of causes which have operated in the lifetime of the individual, and not of inheritance, or, in other words, causes attributable to antecedent generations.

Certain recent biological discoveries, however, have rendered the inductive method the more convenient in disproving the truth of heredity, inasmuch as they are fresh in the minds of those who are interested in this subject. To render our arguments more general and more formal, we shall begin with the fundamental notion of life, on which idea biology as a definitive science depends. Heredity has always been associated with theories of

descent; descent depends on the fact of the succession of individuals, and this again on growth and reproduction. Now growth is, perhaps, the most essential function of living matter, and is the character by which life is best differentiated or defined. As the primary functions will be in constant reference in the arguments which follow a summary account of them may be indicated here. Living things depend on a physico-chemical basis. By means of their physical properties, those physical forces which tend to destroy them are resisted, and they form a framework in which the processes of life are carried on. The chemical properties are exhibited in the method in which the aliment and products of life are appropriated and disposed of. Substances of definite chemical nature are assimilated, and disassimilation also results in certain definite chemical products. The thing which assimilates and disassimilates has a property which no other thing is known to have, that is to say, of appropriating and liberating force and matter, with the constant possibility of increasing its own substance, giving rise to growth and reproduction. These functions of nutrition and growth are the vegetal functions. Motion is characteristic of these phenomena, and seems to suggest that life is in some sense a mode of motion, and this notion will be shown necessary in order to understand the general vital manifestations which constantly confront us. The animal functions are characteristic of animals, and are only in a minor degree exhibited by plants. These are contractility and irritability, by which the animal changes its shape and responds to stimuli. These latter functions appear primarily to be modes of motion along certain lines more definite in nature than those intra-cellular movements that constitute the vegetal functions.

Now these functions are embodied in a unit called a corpuscle or cell. We may assume that this, as the biological unit, may be used universally in our reasoning. It follows, then, that as the idea of heredity existed before the cellular theory was suggested, the former must be adapted to the latter. Heredity was the primitive expression of the relationship of ancestor and descendant from the etiological point of view, and living individuals were then considered the true biological units. It is therefore obvious that if the word "heredity" is to be used, it must be applied to different objects than formerly—to cells, in fact. Then if the word is so applicable, the hereditarians will

be forced to admit that the discovery of the new morphological unit has changed our ideas of heredity. But biologists have shown that our ideas of descent are already changed. They will therefore require to give the word "heredity" a new meaning, if it is to be retained. The irrelevancy of the word to the theory of descent as now phenomenally known may be shown thus:—To establish the nature of the relationship of successive generations, a knowledge of certain cellular phenomena is necessary. The etiological relationship of successive generations is expressed by the word "heredity." But the idea of heredity was established before cells were discovered, therefore it was possible to understand the etiological relationship of successive generations without understanding an essential factor in that sequence, which is absurd.

To show fully the conclusiveness of this argument, I have made a brief sketch of the more simple and elementary truths of reproduction. The subject is divided into two parts—the one referring to the relationship of individuals of succeeding generations, the other dealing with the development of individuals in general. We may examine the nature of the succession of individuals, first as *unicellular* and then as *multicellular* forms of life.

The phrase "hereditary transmission" loses its meaning if applied to the reproduction of unicellular forms of life. It is evident that it could not be applied to these forms in the same sense as that in which it was in the first case applied to multicellular forms; for the way in which reproduction was supposed to take place in these forms has no resemblance to the method in which it takes place in unicellular forms. Whether plant or animal, a unicellular form of life reproduces itself by a process of division into two or more equal or unequal parts. The increase in absolute substance is by growth, and the numerical increase of the living units is by fission, or, in mathematical language, multiplication is by division. This statement fully accounts for the increase of numbers and the preservation in time of such forms of life. It is ridiculously absurd to use the phrase heredity to the phenomena of growth and division of cells. If the higher animals had split up into halves in order to reproduce themselves, would this have been called hereditary transmission? But since this word has been applied to these phenomena, we have an example of how men persist in the use of a word, the meaning of which they do not understand, to explain phenomena to them

equally unintelligible. The internal phenomena of cell division do not lend themselves as a justification for the use of this word.

Again, to say the truth, the word is equally unfortunate when applied to reproduction in forms of life made up of aggregates of cells. Every existing cell is derived from a pre-existing cell, hence every cell in the aggregate that makes up an individual is derived from a pre-existing cell by the division as just described. The denial, therefore, of the truth of hereditary transmission in multicellular forms of life, becomes a corollary of the denial of its applicability in unicellular forms. A review of the process of reproduction in multicellular forms will convincingly show the absurdity of this belief. At the same time it may be well to review the fundamental truths of all genesis in multicellular forms.

In the metazoa the fertilised ovum by cell division becomes a segmentation mass, and, by further cell division, differentiation, and distribution, an adult individual. Now, the cells of which the adult is composed may, when regarded as to their origin and destiny, be divided into two classes—*hypogenetic* and *epigenetic.* Those cells which form the greater part of the body and constitute its several tissues and organs are epigenetic, and those which make up the lesser part of the body and constitute the essential elements of the organs of reproduction, the ova in the ovaries, and the reproductive cells in the testis, and the spermatozoa proliferated from them, are hypogenetic. This classification is foreshadowed by that of differentiated and undifferentiated, or embryonic, as being the most elementary sub-division of animal cells. It will be subsequently observed that the division here proposed is more definite and affords a basis for a variety of biological arguments. The origin of these two classes of cells is the same; their destinies are different. They have both a hypogenetic origin. The ovum gives rise to the epigenetic cells of the body as well as the hypogenetic cells of the ovary. The epigenetic cells form terminal series; they either die in the lifetime of the individual or when the individual dies; while the hypogenetic cells have this peculiar property that they may become separated from the epigenetic mass and give rise to new series, both epigenetic and hypogenetic, and so forth, and again. From this it is evident that epigenesis is interrupted, while hypogenesis is continuous. The hypogenetic cells form an unbroken and independent series, from which, at intervals, epigenetic aggregates are derived, and these latter are

therefore not connected *inter se*. But epigenetic aggregates are as they are viewed, ancestors and descendants, and are not connected *inter se;* therefore descendants are not derived from their ancestors; consequently the idea of heredity is erroneous, for it was supposed that descendants got their essential nature from their ancestors, which they do not.

Hypogenesis is complementary to the term epigenesis, and is descriptive of that part of cell genesis which the latter term does not comprehend. Epigenesis is already an accepted word, and while it is perhaps more scientific it certainly antedates any other word that has reference to the same phenomena.

In refuting these ideas regarding heredity in a more illustrative manner, we shall introduce the argument as to how we show, in the first instance, why individuals that succeed one another in the same hypogenetic line should be like one another. The ova in the ovary of the adult female are more closely related to the ovum from which the individual sprung than any other cells of the body. They are also more identical in nature than any other cells of the body. The latter have become differentiated while the ova are still undifferentiated: each of them is indeed an ovum. During their tenancy of the ovary they merely proliferate and go through a series of transformations which are necessary for the incidents of reproduction and fertilisation peculiar to the species. Now, if we assume the principle which all biologists have, in some sense, already assumed, that like may produce like, it follows that since the ova in the ovary are like the ovum from which they sprung, each of them possesses the possibility of producing such an individual as the original ovum did; and so they do. The individual in which the ova are situated confers no legacy on them; they are only indebted to it for a temporary habitat and aliment. But the ovum, in order that the descent of its nearest kin in the hypogenetic series might be preserved intact, afforded a possibility that both body and soul might be conferred on the individual.

Again, not only are the hypogenetic cells (1) closely related to one another and (2) identical in nature, but (3) they show a very marked distinctiveness from the other classes of cells in the body—three principles, which, by the way, nature has adopted as economical in the art of reproduction. Very early in embryonic development the cells which are destined ultimately to give rise to the spermatozoa, or ova of the adult, are set aside for reproduction. The rudiments of the organs, male or female, can be distinguished

soon after the hypoblastic and epiblastic layers have been formed. And it is remarkable that in several forms of life the cellular genetic sequence can be traced between the ovum, which gives rise to the individual, and the organs of reproduction in that individual. Hypogenesis is, therefore, in these species, a matter of simple observation. This isolation of the reproductive group of cells not only shows how the likeness of succeeding individuals is preserved, but also shows how the ova are protected from any purely external influences. It is equally remarkable, as showing that the reproductive cells are uninfluenced by the other cells of the body, that among the hydrozoa in some cases the reproductive organ becomes detached from the hydrosoma and leads an independent existence from the organism on which it was developed. After becoming detached the zooid cannot possibly be affected by the hydrosoma, so that hereditary mechanics, did they exist, could not come into play. Yet the ova of the zooid produce hydrosoma after the same manner that other ova produce other forms of life. It is just conceivable—but there seem to be no economic grounds for it—that hypogenesis might have been conducted apart from the development of multicellular individuals altogether, and the variations that we see supervene in them still take place. The sequence of events in nature may, at least, be pictured thus, though the detachment is not actually affected. At any rate, the burden of proof that the reproductive elements are influenced while in the body and the contradiction of these reasonings rest with those who believe in hereditary transmission. It seems that the perishable body is merely the vehicle of the triumph of the ova-producing line over time, and that as it subserves this purpose is it good or bad.

That man and the lower animals are but the temporary instruments of hypogenesis, or for the preservation of the reproductive substance through time, is a mode of contemplating life that seldom occupies the mind or finds expression. Still it is as well founded as is it of ancient conception. Aristotle says:—"And such an aim has the vital principle by its nature in living bodies. Thus all natural bodies, those of animals as well as those of plants, are its instruments, and are what they are for its purposes." And again he says:—"The most natural of the functions in beings which are perfect is to produce another such as itself, an animal an animal, and a plant a plant, in order that they may partake to the

extent which has been allotted them of the Everlasting and the Divine. All creatures yearn after this, and for the sake of it they do all that they do naturally; but since such beings cannot in uninterrupted continuity partake of the Everlasting and the Divine, because no perishable being can abidingly continue as one and the same, yet each can partake thereof in its own allotted portion, be it larger or smaller, and still continue if not the same like the same, and one, if not in number, as species." And further, he says:—"Even granting that all things may be from one or more than one primal element, and that the self-same matter may be the source of all beings, yet there is a peculiar matter for each genus." I think that these extracts show, that whatever Aristotle says at other places, he, at least, conceived, among other things, that the living phenomena on the surface of the earth depended on something persistent which underlay that which is obviously ephemeral, and is so for the former's sake.

But rather than digress further from the objects of our inquiry, we shall return to our subject and illustrate our arguments by reference to plant life, which is none the less instructive. We have already shown that the most fundamental classes into which animal cells may be divided are epigenetic and hypogenetic. Vegetable cells lend themselves to the same classification. The hypogenetic cells have been called "meristem," and the epigenetic "fixed tissue;" at least these terms are in general equivalents, just as we remarked as regards differentiated and embryonic cells in animal histology. This classification serves us in comprehending in one view, whether now or in genetic relationship throughout past time, the whole life phenomena upon the globe. It depends on no theory or assumption, but on simple observation.

A plant is for the most part composed of fixed tissue, and this is derived from meristem, which is hypogenetic. But the whole plant was derived from a single cell which by hypothesis was also hypogenetic. The converse of this proposition is not true, namely, that the ovum was derived from the whole plant—for the ovum was derived from meristem, and that from pre-existing meristem. So it may be said that the seed gives rise to the plant, though the plant, as such, does not give rise to the seed. For stronger reasons plants cannot be said to be derived from pre-existing plants as such, but from an unbroken series of meristematic or hypogenetic cells. How then can a plant inherit from that from which it does not spring?

Were we to illustrate the law of the succession of generations in plants, we should have to recall to mind the method of growth and development in these forms of life—how, from a single cell, by cell division, an embryo is formed, and how in this embryo, as in the animal embryo, the cells become disposed in layers; how from these layers branches and roots are sent off, some upwards and some downwards; how the growing point, sometimes a simple, apical cell, keeps constantly in advance, and how the fixed tissues fall behind and become the subjects of decay; how all the members of the plant, roots, stem, branches, leaves, and flowers, flourish and fade so that the growing points may retain their vitality and continue their kind; and how, when the combined efforts of the members of the plant threaten to fail, or to provide against this, the growing points fall to the ground to commence again the reconstruction of similar forms for their own preservation. Thus it is an error to say that the plant sums itself up in the seed.

Such, then, is an outline of the phenomena of the succession of generations, applicable universally, in plants and animals. We have tried to give a proper expression to the bond which exists between individuals, whether closely or distantly related, and we have shown that everything is accounted for by growth and cell divisions, and that there is nothing transmitted in any way. If we are disposed to raise our preconceptions of organic relationships to their foundations and begin with the simpler and pass to the more complex — from the relationships of unicellular forms of life, and then to the relationships of the multicellular forms of life, traced by means of their cellular connections, we shall inevitably admit that changing the cell for the individual as the unit of life wholly alters our conception of descent. We dissever individual from individual, etiologically as well as morphologically, and are therefore driven by necessity to attribute the phenomena of development to causes which operate during development. We have seen that hypogenetic cells form single series, and throughout generations are continuous *inter se;* that the epigenetic groups spring at intervals from the hypogenetic series, and are discontinuous *inter se.* We therefore infer that these groups of cells, or individuals, as they may be called, owe their specific nature to the cells of the hypogenetic series from which they sprung, not to parents or grandparents, as the antecedent groups are called. We may compare the succession of generations to the process of printing. Like a printing press, the

hypogenetic series throws off similar individuals like one another, because of their common origin. They do not get their likeness from one another. We do not say that each print is derived from that which went before; nor should we, for similar reasons, say that individuals are derived from those that have gone before. Successive individuals are produced independently of each other, and owe their likeness to their common origin, and the community of causes which bring about their development.

We now come to the more intimate phenomena during the terms or periods of hypogenesis and epigenesis. It is obvious, if we trace the history of the cells during the development of the embryo, that the cells become differentiated in a very definite way, and to a very great degree. Thus, some cells gradually take on the character of nerve cells, some of muscle cells, others of connective tissue cells, and so on for the various organs and tissues of the body. They also dispose themselves in layers and folds, and follow a very definite marshalling together, no indication or presentiment of which was foreshadowed in the disposition of the hypogenetic line from which they spring. Thus we are supplied with the problem to account for the differentiation and distribution of cells during development—how, from a line of cells that, backward throughout biological time, have been producing ova-like cells should suddenly, and at definite intervals, give rise to a certain variety of kinds of cells quite different from themselves, and disposed in a different manner. If like only produced like it is evident that, even supposing that by fortuitous circumstances these took the form of the body, it would merely consist of a mass of undifferentiated cells, but this is very far from what we observe during the course of development. And if the developmental changes that occur in the embryonic history of the individual are due to forces acting within that time, there must be some principle by which these vital phenomena are rendered intelligible. The following law appears to me to have universal application to the reproductive processes of life: *That cells tend to continue the same and produce their like, and their like only, except in so far as by the agency of external influences they produce cells unlike themselves.* If this law explains the phenomena of life alluded to, the words heredity and hereditary transmission have neither meaning nor application.

The applicability of this law to these phenomena can be shown from a variety of points of view—an argument from analogy

might be adduced. The tendency of cells to produce their like is analogous to that property of a moving body, in virtue of which it tends to go in a straight line. Deflection from the straight line depends on an impressed force. A planet does not pursue a straight line because gravity diverts it. It had been believed that a planet pursued the course it did because that was natural to it, as a circle was the natural figure for a moving body to describe. An ovum, whatever it may be capable of when acted on in a certain way, has no more hereditary tendency to give rise to muscle, nerve, gland, or other cells, than any body has of itself a natural tendency to pursue the course it does. The course a body pursues is the resultant of its own initial movement and the forces impressed on it. So in the same way does the ovum give rise to all the cells in the body. It has a certain initial tendency to produce, under favourable conditions, cells like itself, but differentiation is the resultant of this initial tendency and the forces which have been brought to bear upon the cells. Besides maintaining that the ovum contained in itself all the forces and tendencies required to form the adult, some persons have likewise considered that the ovum itself was a miniature adult; and even at the present time this is the doctrine which gives bias to all the writings on this subject, although it has formally been abandoned. The assumption that the ovum is a miniature adult necessitates very grave logical difficulties as to the origin of these miniatures; so those who throw the causation of individual development upon the ovum remove their problem a little out of the way, but do not solve it. When they come to carry out their arguments to their legitimate end, they arrive at some such conclusion as this:—That the primary or first living unit or ovum contained within itself all the life upon the globe, and that living forms are but selected scraps of that primordial ovum filling up the various habitable spheres in nature.

Whatever it was the foundation of, whatever possibilities it initiated, that primordial mass had no tendency to, had no presentiment of, what it was ultimately to be related to. It is obvious it could only draw its energy from without. From time to time it became a nucleus of more and more complex operations, till at length it formed a basis for the immediate building up of the most complex forms of life. We can scarcely do more than speculate in the vaguest manner as to how these steps have been actually accomplished. The action of external

forces on the organism is denied by no one, and some think that it is the cause of protozoic differentiation, while they attribute the causes of metozoic differentiation to other causes. There are many facts and experiments adduced as illustrating the action of external agencies on living things. Some of these are more characteristic and valuable as showing this than others. The action of light on green plants, whereby actual is converted into potential energy, is an important instance of an external agent being the cause of certain vital phenomena. It is, perhaps, a necessary factor in the causation of these vital changes. The study of the interaction of external and internal forces has some instructiveness as to the origin of vital changes, but its pursuit is difficult. It must always be kept in mind, however, that external forces have been acting through all biological ages, and have produced in a sense a fixed result, and that any influence that we may observe bearing upon an organism lasts but for a short time.

But when we look at cell life we find that the environment of a cell is usually other cells; and so we are forced to study this etiological principle of external causation from a new point of view. In the phenomena of genesis that we have been speaking of it is not the conditions of the individual that determine these phenomena, but the relationships that the cells themselves bear to one another that seem to initiate changes. We may illustrate this point by reference to certain hypogenetic phenomena. In mammals the ovum is developed at the sacrifice of a great many cells which were potentially its equals. Why was this impending slavery entailed upon them? If these ova have the same tendencies and are independent of external influences, why do they not all ripen and secure their fertilisation together? Some determinant must have been acting to produce the result which we see. Whatever initiated this relationship, the ovum comes to exercise a peculiar domination over the cells in its immediate neighbourhood. We can only in general terms account for this phenomenon. Some external influence fell favourably on the chosen ovum and put to its credit the balance of power amongst its co-equals. Hence its survivorship. The nature of the differentiating force has not been definitely ascertained, but this is no reason why its existence should not be recognised.

It may be stated for epigenetic development in general that the various dispositions in layers and the various foldings and

groupings are due to mutual influences that are instituted as each phase of development presents itself.

We thus arrive at the conclusions that an individual is the independent product of forces acting during its lifetime, and owes its nature to these and not to hereditary influences; that individuals that are like owe their likeness to one another in that they have their origin in ova which were like in nature, and that similar developmental forces acted in their construction; that should, during development, these external forces cease, development would cease, the cells would cease to become differentiated and fly off at tangents, so to speak, like a planet were it released from the attraction of gravity.

XV.—*The Anglo-Saxon Poem of Béowulf in the Light of Recent Studies.* By Charles Annandale, M.A., LL.D.

[Read before the Society, 29th April, 1891.]

As is well known, the poem of Béowulf is the longest and most important specimen of the Anglo-Saxon poetry left to us, and also the earliest example of the heroic or national epos in Germanic literature. As such, this poem has naturally attracted much attention since it first began to be studied, about the beginning of the present century; and probably no piece of English poetry of similar extent has been the subject of so much criticism, examination, and discussion. It is not at all an extensive work. It consists only of some 3,180 lines, being thus about the length of four average books of Homer's "Iliad," and not so long as the first four books of "Paradise Lost." Looked upon merely as a piece of literature, the Béowulf has a decided value of its own; but the interest attaching to it is due perhaps less to its intrinsic literary merits, than to the many problems it presents for solution, and to the position it holds in relation to the literary development, the early history, and the mythology, of England, Germany, and Scandinavia. For the poem takes us back to a time probably as early as the settlement of the Jutes, Angles, and Saxons in England; and the nature of the historical, ethnological, and mythical elements it contains, and the views that may be taken of its origin, are such that the Germans and the Danes both claim an equal interest in it with the English. It was a Dane (Thorkelin, in 1815) that published the first edition of the text and the first translation of it, namely, into Latin; and the Danes have done much since to make our knowledge of this difficult poem—which exists in but a single and somewhat imperfect MS.—more complete. English and American scholars have also taken an honourable share in the study and elucidation of the Béowulf, but it is to the Germans that we owe the most thorough and elaborate investigations and studies of the poem, and there is hardly any

point of view from which it has not been treated by them. The Béowulf literature has thus become so extensive that it would be difficult for a person to make himself personally acquainted with the whole of it, scattered about as much of it is in periodicals and *Proceedings* of learned societies, &c., by no means easy to lay hands on. An instructive and fairly full account of the Béowulf, its plot and incidents, with some of the controversies connected with it, is given by Professor Morley in the first volume of his "English Writers;" but, so far as I am aware, the most detailed statement of the various and much divergent views put forward regarding the poem is contained in the "Outlines of the History of Anglo-Saxon Literature," by Professor Wülker, of Leipzig, published in 1885, a work which would be much more widely useful if translated into English. More recently two German works specially devoted to the Béowulf have appeared—"Béowulf-Studies," by Dr. Sarrazin of the University of Kiel; and "Béowulf-Researches," by the celebrated authority on the English language and literature, Professor Ten Brink, both works of over 200 pages. Besides these, a set of essays on the Béowulf, by the late German scholar, Müllenhoff, have recently appeared in their final and collected form.* It thus seemed to me that from these sources valuable material might be obtained which would serve to supplement the information furnished by any English work regarding the great Anglo-Saxon epic.

"Every epic saga," says Müllenhoff, "and the contents of every national epos, consist of two elements—myth and history." Regarding these two elements in the Béowulf very different views have been held as well as regarding its origin, composition, and transmission. Of the historical peoples or communities mentioned in it almost the only one that can be readily and certainly identified are the Danes, who are spoken of in terms of the highest respect, and occupy a prominent place in the poem. Béowulf,

* The full titles of the works above referred to are as follow:—

BEOWULF. — Untersuchungen über das angelsächsische Epos und die älteste Geschichte der germanischen Seevölker. Von Karl Müllenhoff. Berlin, 1889.

BEOWULF.—Untersuchungen von Bernhard Ten Brink. Strassburg, 1888.

BEOWULF-STUDIEN. — Ein Beitrag zur Geschichte altgermanischer Sage und Dichtung. Von Gregor Sarrazin, Ph.D. Berlin, 1888.

GRUNDRISS ZUR GESCHICHTE DER ANGELSÄCHSISCHEN LITTERATUR. Mit einer Uebersicht der angelsächsischen Sprachwissenschaft. Von Dr. Richard Wülker. Leipzig, 1885.

the hero of the poem, however, is not a Dane, but belongs to a neighbouring and friendly people called the Geâtas, being the nephew of Hygelâc, king of the Geâtas, and latterly king of this people himself. In the first of his exploits narrated in the poem, Béowulf sails from his own land to the residence of Hrôthgâr, king of the Danes, determined to destroy a monstrous being named Grendel, who is nowhere clearly described, but possesses certain fiendish or demoniac characteristics, and appears to live on nothing but human flesh. Grendel haunts the neighbourhood of the Danish royal residence (situated probably in the island of Sealand), has devoured a number of the Danes, and has spread such terror that no one dares to encounter him. Béowulf, however, is the strongest of all men, and his courage is equal to his strength. He boldly waits for the monster in Hrôthgâr's great hall, whither Grendel is in the habit of coming at night in search of prey; and after a struggle with him he tears off one of Grendel's arms. But the monster gives him the slip, and escapes to his abode in a swamp or mere at no great distance, where he dies of his wound. The Danes think, with joy, that they are rid of their oppressor. But next night his mother, a monster only less formidable than himself, comes to Hrôthgâr's hall, eager for revenge. Entering the hall, she seizes and devours a sleeping warrior, and makes off to her den. Béowulf, being sound asleep in another apartment, is unaware of this, but when he learns what has happened he resolves to seek the monster in her watery stronghold and slay her next. This resolve he immediately carries out. After a long swim under water he reaches the bottom of the mere, where Grendel's mother dwells in a sort of supernatural cavern that the water does not enter, and after a desperate conflict he slays her and returns in safety. Having thus rendered the greatest services to Hrôthgâr, he is rewarded with the richest gifts, and sails back home. In due time he becomes king of the Geâtas, over whom he reigns for fifty years. At last he slays a fiery dragon that guards a hoard of treasure, and commits great ravages, but dies from the effects of its poisonous bite, thus ending a glorious life by an act of self-sacrifice.

These, omitting details, are the three great actions narrated in the tale of Béowulf, which thus turns on the slaying of Grendel, the slaying of his mother, and the slaying of the dragon. A great swimming match between Béowulf and a youthful rival is also described by the hero himself. Other incidents and events are

briefly introduced or referred to in various connections, and a number of personal and other names are mentioned, often in the most obscure manner.

When we seek to analyse and penetrate a little deeper into the story, various questions at once occur, such as—Who or what was Béowulf? Was he a real man, a product of mythology, or a pure coinage of human invention? Who were the Geâtas? What were Grendel and his mother? To what breed did the dragon belong? and so on. To these questions various answers have been given.

With regard to Béowulf himself, it is open to anyone to maintain that he may have been a real man, although his exploits have a fabulous character. Professor Skeat has taken this view, and has maintained that he was a man who had earned great renown from the slaying of two ferocious bears—namely, Grendel and his mother. The name Béowulf, he believes, is a compound, meaning simply "bee-wolf;" and "bee-wolf," he asserts, is equivalent to "bear," this name being given to the bear in a figurative sort of way, on account of its fondness for honey. Béowulf was thus a a man, called the "bear" from his strength and from his successful encounters with bears.

But though the name Béowulf does seem on the face of it to be a compound meaning "bee-wolf," whether this is the real explanation of it is doubtful. The word does not appear to have been ever used otherwise than as a proper name, and in this poem; and though "wolf" is used with considerable latitude of meaning, for a creature of wolfish habits it seems questionable whether the bear would ever be called a "bee-wolf," thus mixing up the names of two animals so distinct as the bear and the wolf.

Of course, Grendel and his mother might have been originally bears, whatever meaning be attached to the name Béowulf. That they have certain of the characteristics of a bear, or similar beast of prey, may be admitted, but if they were originally such the poet appears to have no consciousness of this origin. They are represented as having something of a supernatural character. They dwell deep down in a mere, in a cavern whence the water is excluded, and which is illumined by a mysterious light; and here—what we would suppose to be of very little use to a bear—there is a store of weapons and war trappings. Grendel and his mother, indeed, seem to be a com-

pound of the fiend, the ogre, and the ferocious animal. In one passage they are spoken of as the descendants of Cain, but this undoubtedly does not belong to the poem originally. To the bear theory one might also object, perhaps, that the killing of a bear or bears would hardly be considered so rare or remarkable a feat as to give rise to an epic narrative like that in the Béowulf. Regarding Grendel as originally a bear, Skeat connects his name with the verb to *grind*, thus making the name equivalent to the grinder or crusher, a derivation which is probable enough whatever view be taken of the character of the monster to which it is attached.

To those who regard Grendel as purely a monster of mythology, Professor Skeat's view would savour of the Euhemeristic method of interpretation, or that anciently used by Euhemerus in regard to the mythology of Greece, and according to which Hercules, for instance, was a real man of enormous strength, who subdued real lions and other wild animals. I do not find that anyone else has taken Professor Skeat's view. Ten Brink, in his recent "Researches," does indeed refer to it, but in a way that shows he has very little respect for it.

The view held by Ten Brink, Müllenhoff, and other scholars is that in the hero Béowulf we have a union or fusion of an old Teutonic divinity with an historic personage. In the genealogies of the Anglo-Saxon kings in the "Chronicles," we find—away back beyond even the remote period of Woden's existence—a Beowa or Beawa mentioned along with others, his progenitors or successors, the most striking member of the series being Sceaf, Sceldwa, Beowa or Beawa, Tætwa. These names are explained as mythological upgrowths of early date, which, when their etymological signification is seen, are found to typify the introduction of agriculture and a settled way of life among the ancestors of the Anglo-Saxons. Sceaf's name being connected with *sheaf*, refers to the introduction of agriculture; Sceldwa, connected with *shield*, denotes the protector, and refers to the position of the king or chief as protector of the people; Beowa, who is the same as our hero Béowulf, has a name which, as explained by Müllenhoff, has nothing to do with bees or bears, but belongs to the root *bhû*, to be, to dwell, to become, to grow (the root of our verb to *be*), referring to settled abiding places and occupations; while Tætwa is akin to Icel. *teitr*, joyful, glad. Although these names appear to refer to different mythological personages, Müllenhoff is inclined to think that they

represent really different sides or phases of one and the same god, and that this god is the northern god Freyr, a god of light and brightness, as well as of agriculture.

If Béowulf is resolved into a god, it is natural that Grendel and his mother should be resolved into something to correspond, and thus Müllenhoff sees in the pair of them personifications of the North Sea. "In spring and towards spring," he says, "the sea rises, a storm floods and overflows the low-lying coast lands, carrying away people from their dwellings and swallowing them up. In this," he goes on to say, "we see Grendel at work, while his mother rather personifies the depths of the seas. Béowulf is the divine being who calms the sea and drives back its floods, leaving the shore dwellers to carry on their occupations unmolested. His peaceful reign of fifty years corresponds with the calms of summer, while the fight with the dragon represents the recurrence of storms in autumn, and this time the god of light, though victorious, meets his own death, and winter reigns once more." In the dragon also he sees the devastating power of water personified, and compares Béowulf's fight with it to that of Thor with the Midgard serpent at the end of the world.

Several other explanations are given of Béowulf's victories over Grendel, his mother, and the dragon; but the facility with which natural phenomena can be brought forward to explain mythology renders one somewhat sceptical as to what is the true explanation in any particular case. As regards the dragon, it would be interesting to consider whether he has anything in common with the gold-guarding griffins we read of in Herodotus, or with the dragon slain by our own St. George.

Sarrazin, who is particularly anxious to trace a Norse origin in everything connected with the Béowulf, is not satisfied with any of the mythological explanations hitherto given. After a discussion of some length and ingenuity, and a display of great familiarity with Teutonic saga and romance, he comes to the conclusion that in the main the exploits attributed to Béowulf are those that belonged really to the Norse god of light, Baldur; while Grendel and his mother he compares with the vampires of South-Eastern Europe, regarding all of them as beings of a demonic character, probably the offspring of the imagination set to work by noxious vapours or exhalations arising from the ground in certain localities. The name Béowulf he regards as the same as that of the hero of an old Norse saga

named Böthvarr, whose adventures he equates with those of Béowulf, and whom he also considers as representing Baldur.

Thus, the continental scholars generally agree in thinking that the main events narrated in the epic are mythological in character, and are based on nature-myths. Professor Morley, on the other hand, reads the story in a manner almost the reverse of this, believing that in the Béowulf we have real history clothed and partly concealed under a mythological or allegorical form. According to him, Hrôthgâr was a Danish chief or king who suffered severely from repeated attacks made on him by an enemy who came from over sea, Béowulf being a chief who lent him powerful assistance. The poet, while recording the name of Hrôthgâr, refuses to transmit to future generations that of the enemy from whose attacks he suffered. The personality of this enemy Professor Morley believes "is lost under the image of a superhuman monster, Grendel. The attacks were from over sea, for which reason Grendel was a water-fiend who came from the bottom of a mere. In the wrestle with Grendel Béowulf defended Hrôthgâr upon his own ground in a successful battle. The second attack, represented mythically as made in revenge by Grendel's mother, was replied to by carrying the war home to the enemy; and this is represented by Béowulf's plunge into the mere. . . . The ocean surge received the warrior, and it was the space of a day before he saw the plain below. This transforms clearly enough into mythical history an expedition by sea to stay ravages of an enemy whom the poet, in shaping the song of victory, commits to oblivion under the mythical form of a strong evil power. In Béowulf's fight, at the close of his life, with the dragon that laid waste his own dominions, there is the same common poetical device. . . . Again, Béowulf battles with a dangerous invader, and while the poet sings the glories of the chief, he denies fame to the strong antagonist, and fashions a more welcome and wondrous tale by fabling him into a fire-breathing dragon."

Such is Professor Morley's theory, as to the probability or improbability of which every one must judge for himself. It might be rash to call it incredible, but one would like at least to have such a poetical perversion of historical facts supported by parallel instances. At anyrate a poet who would adopt such treatment would be far more likely to be one living in an age of artificiality in poetry, and not in such a rude and early period as

this theory supposes. For, if I understand Professor Morley's theory rightly, that the poem describes, in a veiled manner, historical events, I should suppose that he would assign it a date almost or quite as far back as that of the events; and the poet supposed to adopt this treatment would apparently be a contemporary of these events, and would have a personal feeling in the matter. And if he refused to recognise the historical reality of the opponents of Béowulf in some cases, why not in all? Why not treat similarly an event that is several times referred to in the poem, and that is generally recognised as historical—namely, the expedition made by a Scandinavian king, Hygelâc, with a Béowulf in his company, against the Frisians, when Hygelâc fell in battle, and Béowulf himself escaped with great difficulty? If the enemies of Béowulf are allegorised when he is successful, much more would one expect them to be so when he is unsuccessful.*

The defeat and death of the Hygelâc just referred to is the event with which a historical Béowulf—as distinguished from the mythical hero-divinity—is supposed to be connected. Gregory of Tours, in his "History of the Franks," narrates that a certain king of the Danes called Chocilaicus, who can be no other than the Hygelâc of the poem—this name receiving a Latinised form, —undertook a plundering expedition to the Lower Rhine about the year 520, and being met by an army of Franks and Frisians, was latterly defeated and himself slain. "In this battle," says Ten Brink in his "Early English Literature," "a vassal or relative of Hygelâc distinguished himself beyond all others. He seems to have been a man of gigantic physical strength and a skilled swimmer. The fame of this battle, and the glory of this thegn, resounded far and wide among the Geats, Island Danes, and Angles. . . . The deeds of Hygelâc's nephew, the son of Ecgtheow [*i.e.*, Béowulf], were celebrated in songs. Gradually this hero-figure grew to mythical proportions. He entered upon the inheritance of demi-gods. Béowulf, the son of Ecgtheow, took the place of Beowa the vanquisher of Grendel. In England, whither the news of Béowulf and his deeds was borne, presumably

* Professor Morley would explain the name Béowulf as a corruption of *Beadu-wulf* ("war-wolf"), and the new Bosworth-Toller Anglo-Saxon dictionary takes the same view. But apart from phonetic considerations, it seems very unlikely that such a highly-appropriate name for a warrior as "war-wolf" should have got transformed into "bee-wolf," suggestive of something so entirely different.

by Angles, this hero-saga found the soil most favourable to its growth. The son of Ecgtheow was also celebrated in England as the conqueror of Grendel, as the fighter of the dragon." Thus does Ten Brink explain the relation of the historical to the mythical Béowulf, and the spread of the story to England; and though what is stated by him categorically as fact is partly conjecture, most probably it lies close to the truth. The mythical Beowa, of whom the Angles knew, would be confounded with the Béowulf of this famous expedition.

Müllenhoff expresses himself more cautiously regarding the historic Béowulf. What he says is, "We know nothing for certain of the historical Béowulf; but we may, without hesitation, accept it as a fact that a Geat, who bore a similar name to the mythical hero of the Anglo-Saxons, did once really exist, and did in some way distinguish himself as a participator in Hygelâc's last expedition, or on some other occasion, because otherwise we should be quite unable to explain the association of the myth with the historical event."

As regards who Béowulf's people, the Geâtas, were and where they lived, there have been several opinions. They were, in all probability, however, a people of South-West Sweden, the modern Götaland or Gothland. This is the view generally held of the locality to which they must be assigned, and is the one supported by Grein, Müllenhoff, Wülker, Ten Brink, Sarrazin, and others. Müllenhoff remarks, "Kemble looked upon these Geâtas as Angles, but there is nothing to support this view; the Angles are nowhere named in the whole poem. The correct explanation was first given by Ettmüller; the Geâtas are the Old Norse Gautar, the people of modern East and West Götaland in Southern Sweden, who were mentioned long ago by Ptolemy and Procopius as Gautoi." The Geâtas are spoken of by some writers as Goths, and apparently are identified with the people so well known in history under that name. But there seems no good ground for such identification, since the historic Goths are not known to have ever inhabited the Scandinavian peninsula. At the earliest period at which we became acquainted with them they are found dwelling to the South or South-East of the Baltic.

Another view regarding the Geâtas is that their name identifies them with the Jutes, and that in the poem they are to be understood as dwelling in Jutland. But the most recent writers, as we have just seen, deny that they were the Jutes, and though there is

a similarity in the names, there is considerable phonetic difficulty in identifying them.

That the main events narrated in the Béowulf are supposed to take place in Denmark, more especially the island of Sealand, or in Sweden, very few seem to have doubted. One attempt has been made, however, to transfer the whole to English soil, namely, by the Rev. Daniel H. Haigh, in a work called "The Anglo-Saxon Sagas," published in 1861. Professor Morley speaks of this theory with some favour, but hardly anybody else seems to do so. Ten Brink characterises Haigh's attempt to trace English history and English localities in the Béowulf as "quite a failure" (*durchaus verfehlt*), and Wülker treats it with as little respect. Haigh's identification of place-names often rests on the very slightest grounds, and one would sometimes think that he was animated by a spirit of perverse ingenuity and desire to be singular. For my own part I can hardly imagine any one reading Béowulf and thinking he was reading of events and localities belonging to Anglo-Saxon history and the topography of England.

When we come to consider the literary history of the poem and what relates to its origin and composition, we find the greatest diversity of opinion to prevail, the chief controversies being as to where and when it originated, whether it is to be regarded as primarily Scandinavian and secondarily Anglo-Saxon, whether it is the work of one or more hands, &c. Seeing that the Anglo-Saxons are never once mentioned in the poem, but that it sings the praises of the Danes and Geats, the idea that would most naturally occur would be that the poem was, originally at least, Scandinavian. This, indeed, has been from first to last a common and well-supported belief, though no Scandinavian prototype can be referred to. Thorkelin, in his original edition of the text, describes it as "Poema Danicum dialecta Anglo-Saxonica," a Danish poem in Anglo-Saxon dialect; and Sarrazin, at the present day, argues strongly in favour of much the same view. He is also a supporter of the unity of the poem, which has been frequently impugned, in particular by Müllenhoff, and by Ten Brink in his recent "Untersuchungen." Sarrazin even thinks he can point to the actual author of the ancient Danish original, as also to the Anglo-Saxon translator and adapter. He finds in the Anglo-Saxon a number of words and expressions which, in his opinion, betray an old Norse origin, being either borrowed directly from the old Scandinavian poem, or imitations of old Norse forms. These go to

prove, he thinks, that our Béowulf is a pretty accurate translation of a lost old Danish poem. Competent scholars, however, do not admit that these alleged Scandinavianisms have any great weight as evidence. Of the unity of the poem—a point on which different critics are bound to judge differently—he is convinced from the uniform unbroken similarity of the narrative style which he finds to prevail all through; and he does not believe, as do Müllenhoff and others, that the dragon story did not form part of the poem from the beginning. At the first glance, he admits, the tone of the second part—that is to say, the dragon story—seems different from that of the first. It is less rich and varied in some respects, he admits, more monotonous, has more monologue and less dialogue, as well as less description of scenery; but this he explains by the Danish poet having a slight acquaintance with the Gautish localities in which the scene is laid, and with Gautish affairs, and by the character of the subject; and between the two there is after all, he asserts, a great similarity in tone and style. Such inequalities, contradictions, &c., as others point to in proof of the poem having proceeded from several hands, are not to him sufficient evidence of this. Contradictions might be pointed out, he says, in the works of the best writers, and could we expect greater correctness on the part of a poet who certainly could neither read nor write? Sarrazin admits that the work may be based on several older songs, and he also admits, what I believe every one does, that the poem has been subjected to extensive interpolation, the most characteristic interpolations consisting of moral or edifying reflections and remarks bearing on Christian doctrine, inserted with the intention of toning down the heathen elements of the poem.

The Danish poet, Sarrazin, believes him to have been a skald, or professional poet, basing this belief chiefly on the fact that openhandedness on the part of princes is so much insisted on in the poem. The particular skald whom he fixes on was one named Starkad, whose name occurs in Danish saga (but whom Müllenhoff regards as mythical); and the poem, he thinks, was written about the year 700. The Anglo-Saxon version of the poem he attributes to the famous poet Cynewulf, the author of some of the finest Anglo-Saxon religious poems; but how the Danish poem reached Cynewulf he does not explain; and since the original poem, according to his own statement, was by a poet who could not write, some further explanation seems needful on this point. He fixes on Cynewulf as the translator and adapter on account of resemblances in style

and similarities of expression that he finds in the Béowulf and the poems attributed to Cynewulf; and he makes Cynewulf himself the interpolator of the poem on the following theory:—Cynewulf, he says, produced the Anglo-Saxon version in earlier life, when he was a wandering singer, and he followed closely the Danish original, probably adding materials from other sagas. In later life he became a member of some religious body, and going over again the work of his earlier years, he added moralising and theological scraps, to give the poem something of a Christian character, and justify to himself his giving it to the world anew.

The strongest objection to Sarrazin's or any other theory—such as Morley's—that assigns an early Norse origin to the Béowulf seems to rest on the pervading spirit and tone of the poem, the refinement and humanity of sentiment, the note of devotion to duty and of self-sacrifice which it displays. Ten Brink takes this view, maintaining that the Béowulf, though essentially a heathen poem, must yet have taken form and character under Christian influences, and amid social surroundings different from those to be found in Scandinavia at the period to which we must assign our epic. At the time when Sarrazin supposes the poem to have been composed, the Danes and their neighbours were heathens, and if they were not yet so notorious for their piratical expeditions as they afterwards became, the spirit of the viking and the baresark must have been predominant amongst them.

While Sarrazin and others contend strongly for the unity of the Béowulf, Ten Brink, following Müllenhoff, is equally strong on the other side. Müllenhoff believes six hands in all to have been concerned in the production of the poem. The two oldest portions, he thinks, are those describing the fight with Grendel and the fight with the dragon; these are by two different authors. To the Grendel tale was afterwards added, by a third hand, the slaying of Grendel's mother; while a fourth completed this portion of the poem by adding the general introduction. A fifth now added a continuation, consisting of the section describing Béowulf's return home after slaying Grendel and his mother, and this poet also made certain interpolations. To the portion thus built up (that is to say, introduction, Grendel, Grendel's mother, and Béowulf's home-coming) the sixth hand added on the old dragon song, and inserted a great many interpolations, partly of the character of those already described, but partly brief episodes from sagas having more or less connection with the Béowulf story.

Müllenhoff goes over the whole poem, and points out what is original and what is interpolation, and the result of his investigation is to stigmatise some fourteen hundred lines as interpolations, and not part of the original text. Professor Morley speaks in very slighting and sceptical terms of Müllenhoff's attempt to analyse the poem into its different elements; yet Müllenhoff's results were not finally arrived at till he had given twenty years' study to the poem, and that in itself would seem to entitle his views to be considered with some respect.

At anyrate, Ten Brink is at one with Müllenhoff in refusing to admit that the Béowulf is the work of a single author, and he finds it to consist essentially of the same component parts, originally separate, but latterly combined. He maintains that it is impossible to believe in the unity of the poem; and what Sarrazin regards as trivial discrepancies and imperfections he designates as intolerable repetitions, contradictions, and "crass *hystera-protera*," which are only to be explained by the coming together of heterogeneous elements.

According to him, the work is English and not Scandinavian, so far, at least, as it is a work of literature. The mythological and traditionary matter which we find in it was brought from the Continent, but the epic developed itself on English soil, and mainly under the influence of Christianity, which gave it the prevailing tone of refinement and humanity already referred to. The different sections of the poem would take form separately in the mouths of popular reciters, and some of them would assume an epic shape sooner, some later, than others. Several different versions of the same incident would also arise, which, while generally the same in matter, would differ considerably in details as well as in language. He believes that we owe the Béowulf in substantially its present forms to a general-editor or redactor who fused together different versions of the component parts of the tale, trying to use up as many lines of each version as seemed to him of any value. He would have before him, say, an earlier and a later version of the slaying of Grendel, and he would try to blend both together, so that the result would be so many lines of the one, so many of the other in succession. He would not do his work very skilfully, however—he would not, like the modern editor, be able to avail himself of paste, scissors, and printed texts,—and so many repetitions, dislocations, &c., would naturally occur. On this hypothesis Ten Brink goes carefully over the

whole poem line by line, trying to point out the source from which each successive portion was derived, and to show how what he regards as chaos may often be reduced to order—without, however, intending to insist that all his suggestions hit the mark.

The part played by the old redactor on this theory would somewhat resemble what has been done by some editors of Scottish ballads, who have had several versions longer and shorter to work on, and have sought to produce a sort of eclectic version by selecting stanzas and lines from each. Ten Brink does not believe that this hypothetical general-editor was a poet himself, or that he added anything of his own worth mentioning, though interpolations were subsequently inserted. The older portions of the poem he would place about 600-650 B.C., the younger not long before 697 : the general redaction probably belongs to the eighth century.

Wülker also agrees substantially with Müllenhoff in his analysis of the poem into old songs, but he believes that these were taken in hand not merely by an unpoetic editor but by a poet, who made free use of them, and added material from other sagas, thus producing what may be considered an independent poetic whole. "It is quite certain," he says, "that the song of Béowulf, as we possess it, is based upon older songs. That the fight of Béowulf with the dragon was originally a song by itself no one will venture to dispute. I quite agree with Müllenhoff who, in the fight with Grendel, and the fight with the dragon, recognises two independent poems, which are at the same time the oldest of the Béowulf songs; also, I hold as very probable, that Béowulf's fight with Grendel's mother is but a continuation of the fight with Grendel himself, and, consequently, younger. Later singers, when they saw that their heroes and their adventures roused the interest of their listeners, continued to add further adventures, and the same was done by a singer in this case." As a piece of evidence tending to show that Grendel's mother came into existence later than Grendel himself might be adduced the fact that she has no name of her own, her personality thus, in a manner, depending on her relation to her son.

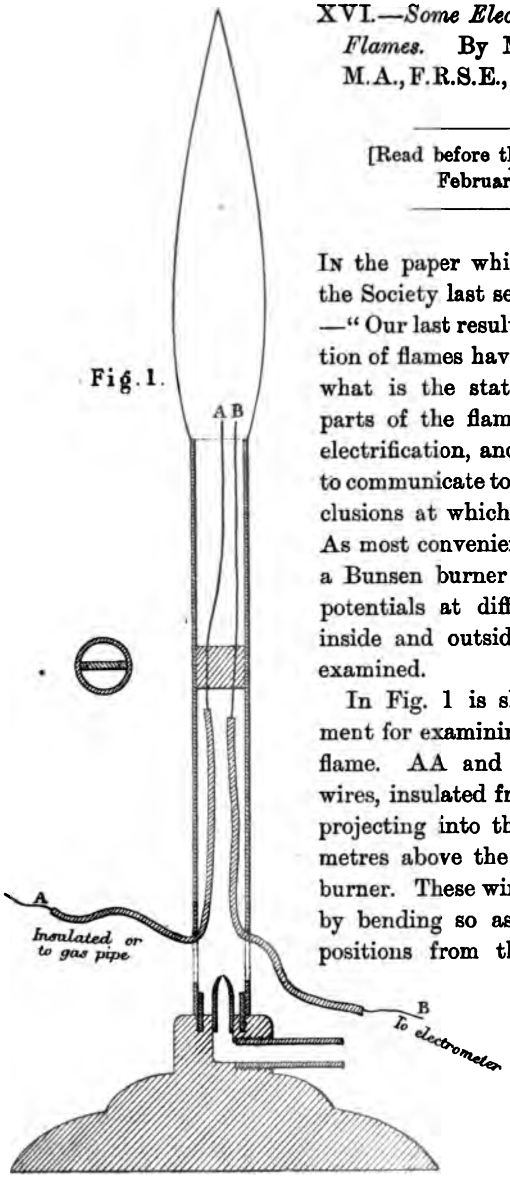
Professor Earle, in his admirable little work on "Anglo-Saxon Literature," published in 1884, expressed a belief that the poem belonged to the close of the ninth or beginning of the tenth century, "that the Saxon poet got his story from a Dane," and that the earliest probable time at which this might have happened would be after the peace of Wedmore in 878. Since then,

however, Professor Earle has entirely changed his view of the date and origin of the poem. He would now assign it to the latter part of the eighth century, and believes that, though based on legend or tradition and partly on history, it was written expressly with a didactic purpose, to serve as "the institution of a prince," the particular prince for whose edification it was intended being Ecgferth, son of the great Offa, the Offa who built the celebrated "dyke." His new views were first set forth in three letters to *The Times*, but we shall soon have an opportunity of becoming acquainted with them in their matured form when his translation of the Béowulf, already announced, makes its appearance. That the poem was written with a didactic purpose is a hypothesis that might be supported with plausible enough arguments, and I have no doubt Professor Earle's new theory will commend itself to many.

XVI.—*Some Electrical Properties of Flames.* By MAGNUS MACLEAN, M.A., F.R.S.E., and MAKITA GOTO.

[Read before the Society, 18th February, 1891.]

Fig. 1.



IN the paper which we read before the Society last session we remarked —" Our last results on the electrification of flames have led us to examine what is the state of the different parts of the flame itself as regards electrification, and we hope later on to communicate to the Society the conclusions at which we have arrived." As most convenient for our purpose, a Bunsen burner was used, and the potentials at different points both inside and outside the flame were examined.

In Fig. 1 is shown the arrangement for examining the inside of the flame. AA and BB are platinum wires, insulated from the burner and projecting into the flame, five millimetres above the upper end of the burner. These wires can be adjusted by bending so as to lie in various positions from the middle line of the flame to its boundary. The Bunsen burner itself was in every case connected to earth, so that by leaving one of the wires free in air, and joining the other one to one of

the terminals of a Thomson quadrant electrometer, while the other terminal was joined to earth, we could get the difference of potential between the earth and every point of the flame from edge to edge, and five millimetres above the edge of the Bunsen. Or, again, one wire was joined to one terminal of the electrometer, and the other wire to the other terminal, and thus we could get the difference of potential between any two points of the flame itself, along the line five millimetres above the Bunsen. The electrometer was set up in the usual way, with lamp and scale, and the sensitiveness of it was such that a difference of potential of $\frac{1}{200}$ volt between its terminals could be observed.

In Fig. 2 is shown the arrangement for examining the outside of the flame. C is a platinum wire fused into a glass tube which covers the wire, except a very small portion of its end. D is the scale for measuring the distance of the point of the platinum wire from the boundary of the flame. E is the index. Thus, by joining, as before, the Bunsen and one terminal of the electrometer to earth, and joining the other terminal to C, we could find the difference of potential between successive points outside the flame, on a line five millimetres above the edge of the Bunsen, and earth.

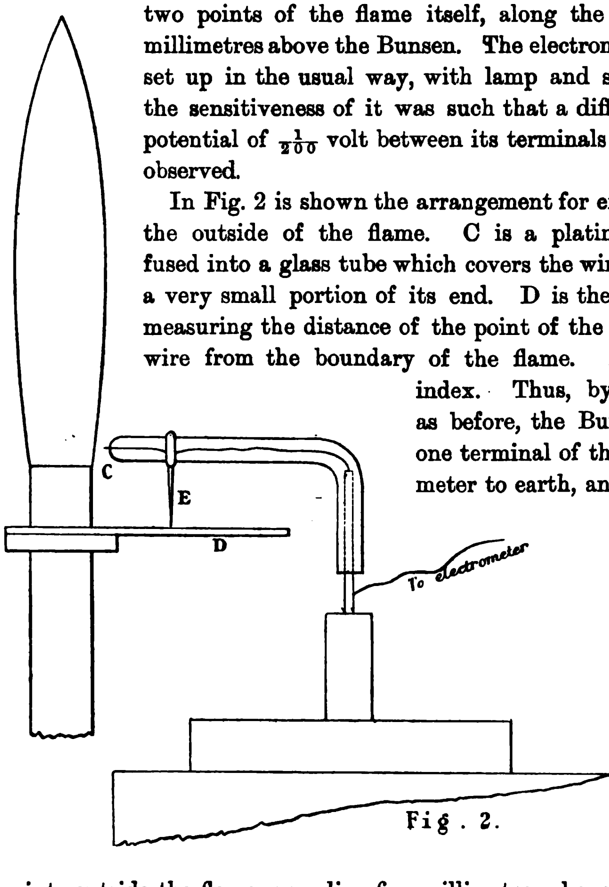
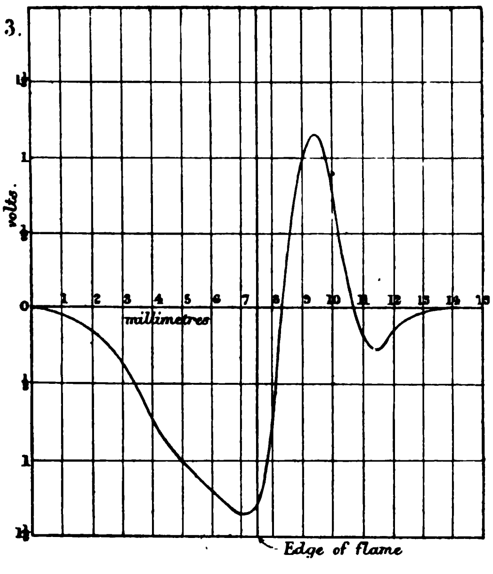


Fig. 2.

By these arrangements, it was found that the flame is negatively electrified, while the film of air surrounding it is positively electrified. These results had already been obtained by Elster and Geitel. Our results agree with what they found, though our

method of examining the different parts of the flame is different from their method. (See an abstract of their experiments by S. P. Thompson in *Nature*, Vol. XXVI., No. 666.) Fig. 3 shows the curve of potential obtained after several observations. It will be observed that the potential at the middle line of the flame is nearly zero, and the maximum negative potential lies just inside of the flame, while the surface of maximum positive potential lies at a distance of two millimetres from its boundary. The maximum difference of potential between these two surfaces was found to be between $2\frac{1}{2}$ and 3 volts.

Fig. 3.



Other flames were tried and similar results were obtained; but in the case of glowing charcoal the point of the platinum wire had to be brought very close to it, in order to obtain the indication of positive potential of the film of air surrounding it.

Having thus made quantitative measurements of the potentials of different parts of a flame, it was expected that the magnitude of the electrification of the air would depend on the part of the flame that was earthed; that is to say, that maximum positive electrification would be obtained when the most negative part of the flame was earthed, and *vice versa*. This expectation was verified

by placing the Bunsen burner 3 feet below the nozzle of a water-dropper, connected up in the manner described by us in our paper on "Electrification of Air by Combustion," read before this Society last session, and printed in Vol. XXI. of the *Proceedings*.

The flame of the Bunsen burner was next surrounded by a wire-gauze cage, which was made to lie on the surface of the flame. The burner and wire-gauze were connected to a gas-pipe for earth. Under this arrangement, the effect of the flame was just like that of glowing charcoal, the electrometer connected to the water-dropper showing 6 volts positive.

Another cage, open at the top, and connected to earth, was made to lie on the surface of maximum positive potential surrounding the flame. This was difficult to do, and only a part of the cage fulfilled the condition. In this case, the air was electrified to 2½ volts negative, while the burner without the cage electrified the air to less than 1 volt.

These experiments help to explain why glowing charcoal electrifies air positively, for it acts like a solidified flame connected to earth. For a similar reason spongy platinum, when it glows in hydrogen or coal-gas, is negatively electrified like glowing charcoal.

In our experiments it was desirable to know the maximum electrifying effect of the flame, and this was found by the following method:—The flame to be tested is placed on a stand about 3 feet below the nozzle of the water-dropper, which is connected to the electrometer. The room is electrified, say negatively, to a certain potential, which is indicated by the electrometer, by turning an electrical machine in the further end of the room. The flame to be tested is then lighted, and the change of deflexion of the electrometer, if any, is noted. If the change of the deflexion is towards negative, the flame is put out and the room is further electrified negatively by turning the machine; but if the change of the deflexion is towards positive, we wait for some time till the electrification of the room decreases by some scale-divisions, and the flame is again tried to see the effect. This process is continued till no change is observed on lighting the flame. The reading at that time is taken as the maximum effect of the flame. The following observation on a spirit-lamp connected to a gas-pipe may be taken as a specimen;—

Deflexion before lighting.	Deflexion after lighting.	Direction and amount of change.
30	20	+10
15	13	+ 2
13½	12	+ 1½
12	11	+ 1
6	8½	− 2½
8½	8½	0

The maximum effect is therefore 8½ divisions, equal to 0·6 volt negative, 14 divisions of the scale corresponding to one volt.

When this same lamp was put on an insulating stand and connected to the negative pole of one Daniell's cell, the other pole being connected to gas-pipe, the maximum deflexion was 40 scale-divisions (= 3 volts), and when two Daniell's cells were used in

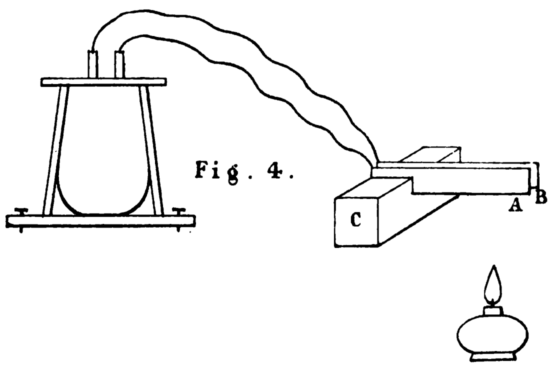


Fig. 4.

the same way the maximum deflexion was 60 scale-divisions (= 4¾ volts).

Experiments the arrangements for which are shown diagrammatically in Fig. 4, were also tried. A and B are metal plates supported about 2 millim. apart by the insulating material C, and joined by wires to the terminals of the electrometer. A spirit-lamp is placed about a foot below, so that the hot air from

the flame passes between the two metals. Differences of potential produced by this arrangement are given in the following table:—

Metal positive relative to other metal.	Metal negative relative to other metal.	Deflexion.	Difference of potential in volts.
Polished Zinc B.	Polished Zinc A.	2·0	·04
Unpolished.	Polished Zinc.	17·0	·32
Unpolished Copper.	Polished Copper.	1·6	·03
Polished Copper.	Polished Zinc.	41·6	·78
Platinum.	Polished Zinc.	43·6	·82
Platinum.	Polished Copper	9·0	·17

Hot air from the flame seems to have a different property from ordinary hot air, because the hot air from a large red-hot soldering bolt, put in the place of the spirit-lamp, had no effect; nor had breathing upon the plates, nor the vapour from hot water any effect.

XVII.—*The Great Winter: a Chapter in Geology.* By DUGALD BELL, Former Vice-President of the Geological Society of Glasgow.

[Read before the Society, 15th April, 1891.]

GREENLAND.

IN these days of adventurous expeditions, one of the most remarkable has been the crossing of Greenland, from the east coast to the west, by the young Norwegian explorer, Dr. Nansen, and five hardy comrades, in the autumn of 1888. The region which they traversed was in lat. 64° to 64½° N., where the country is about 260 miles in breadth. From the descriptions given by Dr. Nansen in the interesting account of his journey which has been published *—and from those of previous explorers, who had penetrated to some distance from other points of the coast,—we can form a tolerably vivid idea of what the interior of Greenland

Fig. 1.—From Nansen's "Greenland." Vol. II., p. 16.
(By kind permission of the publishers.)

is like. It consists of a vast, dome-shaped plateau of ice and snow, with an extreme height, where crossed, of between 8,000 and 9,000 feet, rising northward, however, to a still greater elevation. The surface is firm snow, or loose granular ice, which at no great depth becomes a solid and impenetrable mass. In some parts, particularly near the coast, it is much broken by fissures or "crevasses," but in the centre, for long distances, it seems perfectly level, or with only "long, gentle undulations, scarcely discernible to the eye." So vast is this accumulation of snow and ice that the valleys are filled up by it, and the mountains buried beneath it, only here and there some higher peak rising, like a little island, above the wide frozen waste.

* London: Longmans, 2 vols., 1890.

League after league, far as the eye can reach, it presents the same aspect of monotonous desolation—cold, silent, motionless. But wholly silent it is not, for, on listening close to the ice, one hears "a peculiar subterranean murmur from the streams enclosed below," while now and again "a loud report announces the formation of some new crevasse." And motionless it is not, for this great body of inland ice is constantly, steadily pressing outwards from both sides of the main axis of the country, finding egress by all the valleys and fiords that open on the coast, and reaching the sea as immense glaciers, many miles in width, from which icebergs are constantly breaking off and floating away southwards into the Atlantic.

Our Own Country.

This picture of Greenland is of interest to us as the nearest representation we can point to in existing Nature, of what geologists make out the condition of our own country to have been at a comparatively recent period. Here also—if their interpretation of the facts may be trusted—the hills and valleys were all buried under a thick covering of snow and ice, only the tops of the higher mountains being left visible; and a great ice-sheet extended on all sides of the chief mountain ranges, pressing its way continuously outwards by every valley, and firth, and arm of the sea. This Great Winter, or "Age of Ice," seems to have been an *episode*—differing much from what preceded and from what has followed,—a very wonderful episode, in the geological history of our country, and of neighbouring parts of the world. The question then arises, upon what evidence, and by what steps have geologists been led to this at first-sight incredible conclusion?

It has been made out by a long series of observations and inferences, and its establishment as a truth in geology forms one of the most striking chapters in the history of science.*

The Phenomena.

The basis or starting-point of the whole investigation consists of those superficial deposits commonly known as the "drift" or

* A voluminous literature has grown up around this subject during the past forty or fifty years, in Switzerland, Italy, Germany, France, Norway, as well as in this country and in America. Some seventeen years ago, Dr. James Geikie, in preparing his well-known work on "The Great Ice Age," began to collect materials for a bibliography of the subject, but left off, as he found it would require a volume to itself. Since then, we suppose, the list has more than doubled

"boulder-clay"—those accumulations of stones, clay, sand, and gravel strewn irregularly all over the country, from the sea-level up to a height of over 2,000 feet; also certain features of the rocks—the rounded, smoothed, and striated surfaces which they almost everywhere present; together with the "erratics" or "perched blocks"—evidently travelled masses, and often remarkable from their position—which abound in many localities.

The problem was—How to account for these phenomena? by what means or natural agency were they produced?

It is to be remarked that the phenomena are of a very complex nature, and that only gradually were their leading characteristics clearly distinguished, and their significance understood. A brief summary of them may enable us to see more clearly how several of the theories at first propounded had shortly to be set aside as inapplicable.

(1) BOULDER-CLAY.

Full of stones of all sizes, large and small, confusedly mixed together.

These stones of various kinds, derived from sources near and remote. As a rule, those of a near origin are the more abundant. Some of the others have evidently been brought from considerable distances, and over various obstacles.

They are mostly angular or sub-angular, flat or oblong, smoothed and polished chiefly on one side, and showing scratches or striæ almost invariably in the line of their longer axis. (Fig. 2.)

The clay in which they are imbedded is usually abundant, in some cases forming as much as 70 per cent. of the whole deposit.

It is largely local, taking its colour from the immediately underlying rocks, and varying as the deposit passes from one series of formations to another.*

In general it is unstratified and unfossiliferous; fragments of shells and other organisms found in it in some places have evidently been taken

* In the boulder clay around Glasgow the general proportions of the different kinds of rocks were noted by Mr. Smith of Jordanhill, many years ago, as under:—

	Per cent.
White sandstone and shale (from neighbouring coal strata),	60
Trap (from Kilpatrick hills, &c.), - - - - -	30
Clay-slate, and other schistose rocks (from Dumbartonshire and Argyleshire mountains), - - - -	10
Granite (from a greater distance), - - - -	1

Other observers in the Clyde district give the materials as about two-thirds local, and one-third from a distance, which corresponds very closely with Mr. Smith's figures. Around Edinburgh a still larger proportion are local; from five to twenty per cent. have come from the Ochils, and two to five per cent. from the Highland mountains.

up and transported along with it by the agent, whatever it was, which formed the deposit.

Fig. 2.—Striated Stone from Boulder-clay, Kelvinside.

(2) ROCK-MARKINGS.

The rocks immediately underlying the boulder-clay usually show fine lines and striæ, bearing in the direction from which it is evident the stones have been transported.

These striæ are wonderfully straight and parallel, going, in some instances, into all the inequalities of the rock, and continuing even on vertical and overhanging faces.

The rocky knolls are very generally dome-shaped or *moutonnées*, with a smooth sloping side in the direction whence it is evident the abrading force came, and a rougher side in the other direction *(Stoss seite and lee seite)*. This is noticeable all over the country, from sea-level up to a height of between 2,000 and 3,000 feet. (Fig. 3.)

The striations and *roches moutonnées* radiate in all directions from the main mountain-masses of the country. (See Map, Pl. I.)

Fig. 3.—*Roches moutonnées* and perched block.
Direction of ice flow <————.

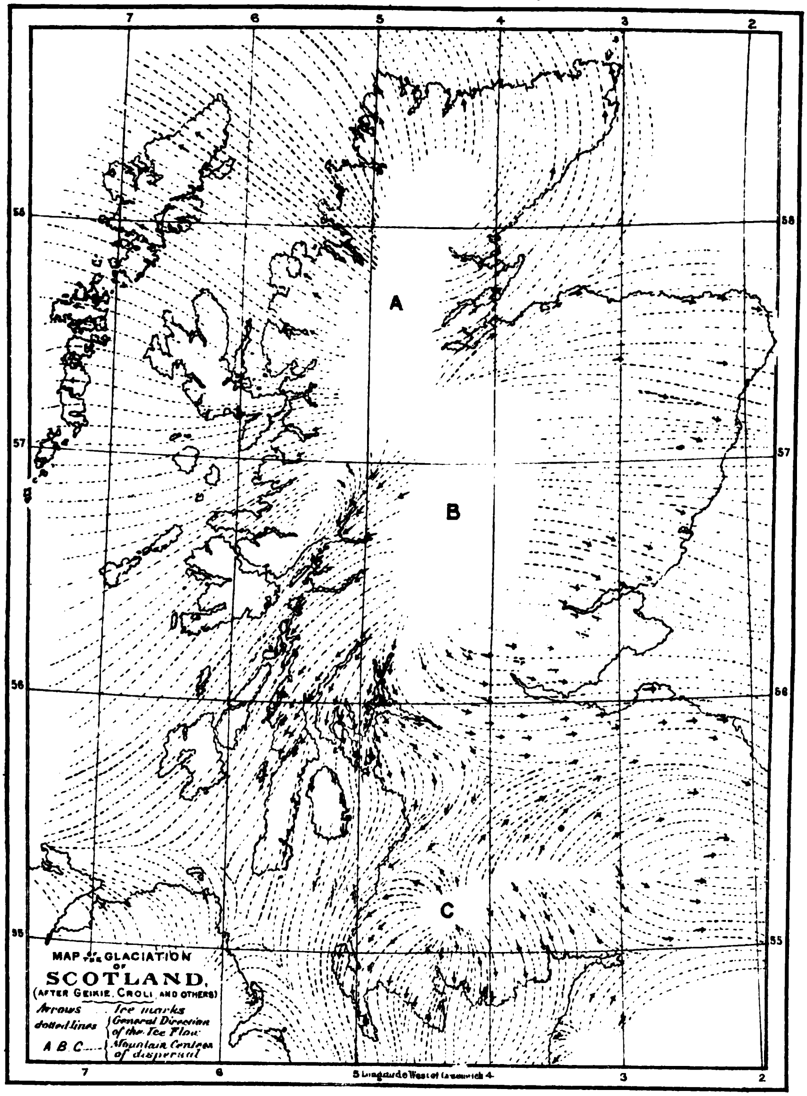
(3) PERCHED BLOCKS

Occur in many places, poised on narrow ledges and detached knolls, from which they might easily be dislodged.

In general, the striations upon them and the position of their longer axis correspond in direction with the striations on the subjacent rocks.

There are certain areas occupied by blocks belonging to particular groups of mountains, from which those belonging to neighbouring groups have been excluded.

Some of the largest blocks have been transported to the greatest distances.



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Special Causes.

As these features were gradually brought to light, it became quite clear that the earlier theories were untenable.

I. "Great floods," or "waves of translation," for example, it was seen could not meet the case. Moving water tends to arrange and stratify the materials on which it operates, carrying the lighter and finer kinds to greater distances, and the coarser and heavier to less; and currents powerful enough to move large stones must have swept off the abundant fine clay in which they are imbedded. Then, no such agents could produce the fine regular markings on the subjacent rocks, or the longitudinal striæ on the stones in the clay, or transport huge boulders to such distances and over such obstacles as they have surmounted, and leave them in the positions in which they are now found. Besides all this, there was the difficulty of accounting for these agents themselves—by earthquakes, the bursting of mountain lakes, and other imaginary hypotheses. But we need not further discuss what has long since been abandoned.*

II. The theory of "icebergs," however, still lingers in the minds of some geologists. It is not so imaginary as those which we have just noticed. It is a fact of everyday occurrence, in some parts of the world, that icebergs break off from glaciers that reach the sea, and drift away, carrying more or less *débris*, which they drop upon the sea-bottom in their progress. So, at first sight this might seem a likely enough explanation of the phenomena.

But the more it was considered, and the more carefully the phenomena were studied, the more inadequate, in all respects, did

* These theories were largely due to an "imperfect induction of particulars," which misled the ablest writers. Thus Dr. Whewell, in the last edition of his great work on the "History of the Inductive Sciences," remarks that "the boulders are of smaller size as they are found more remote" from their parent rock, and that this rather suggests "the notion of currents of water as the cause of the distribution of the materials." (Ed. 1857, vol. III., p. 457.) But the fact is not as stated, or it is only partially true; a leading characteristic of the "drift" being the mingling of large and small stones together; and many of the largest blocks have been carried farthest—as the *Pierre à Bot* in Switzerland, the mica schist boulder on Hare Hill, in the Pentlands, and the "Baron's Stone" of Killochan, Ayrshire. Such instances sufficiently negative the "notion of currents."

this explanation appear. It became evident, for example, that icebergs could not produce the fine regular striæ and smooth rounded surfaces on the subjacent rocks; nor account for these markings diverging from the main mountain centres of the country; nor cause the mingling of near and distant kinds of stones in the boulder-clay; nor leave the perched blocks in the localities and positions where many of them are found. If produced by icebergs, the markings would undoubtedly run parallel to the coasts along which the ocean-currents bearing these deep-seated frozen masses flowed. The boulder-clay would also, in that case, be, as a whole, stratified and fossiliferous; and the stones in the clay would be mostly, if not entirely, foreign to the localities in which they have been laid down.* Sir C. Lyell admitted this—that detritus deposited by icebergs "will have no necessary relation to the hills, valleys, and river-plains, over which it will be scattered."† But the fact is, as we have seen, that the great majority of the stones in the boulder-clay—about two-thirds of the whole—are quite local, indeed, from the immediate neighbourhood, and the clay itself takes its colour and aspect mainly from the same near source.

The general uniformity in position of the perched blocks, also, as noted by many observers, the correspondence of their longer axis with the striæ on the rocks, and with the direction in which

* It is obvious that sediment dropped from icebergs must assume a more or less stratified form by the action of the currents in which the bergs are moving. Sir C. Lyell saw this difficulty, and supposed that the berg might run aground, and be stationary; and then, he said, "if there be no current, the heap of angular and rounded stones may fall to the bottom in an unstratified form called *till*." ("Antiquity of Man," 3rd Ed., p. 231.) "If there be no current"! But how did the iceberg get there, and how did it run aground, without a current?

"It is physically impossible," said Dr. Croll, "that any deposit formed by icebergs could be wholly unstratified." ("Climate and Time," p. 437.)

Again, if the boulder-clay had been formed by droppings from icebergs, it would undoubtedly have contained various organisms. The absence of these in the boulder-clay (except fragments which have evidently been carried into it by the ice) was at first explained by Sir C. Lyell by "the severity of the cold, and also by the depth of the sea, during the time of extreme submergence." But he afterwards stated that his faith in this explanation had been "shaken by modern investigations, an exuberance of life having been observed both in arctic and antarctic seas of great depth, and where floating ice abounds." ("Student's Elements," 1871, p. 155.)

† (Lyell's "Student's Elements," p. 150.)

they have evidently come, could not be imparted to them by icebergs, which, on the contrary, would drop or leave their rocky burdens in every conceivable position, not one in a hundred, probably, coinciding with the direction in which it was being carried.*

Other difficulties are connected with the submergence which the theory requires. This, its advocates admit, might be as much as 3,000 feet.† There is no evidence of anything like such a deep submergence of the country in recent geological times; all the undoubted post-tertiary shell-beds, both in this country and in other parts of the northern hemisphere, are within the 500 or 600 feet line; and the few patches of shelly gravel found at higher levels (1,300 or 1,400 feet) bear every mark of not being in place, or as laid down by the sea. There is really no other evidence; and if we can suppose such a submergence, it is plain there would be nothing of the country left above water but a few narrow detached summits, a few groups of small islands, quite insufficient to form glaciers, or, consequently, icebergs of any size, besides the great probability that, in such conditions, the climate would be so mild and equable that no ice would be formed at all. In either case, the icebergs must have been of foreign origin, drifting to the shores of our country from some other northern land. But this, again, the prevailing local character of the boulders and clay will not permit us to believe.‡

Even if we grant the submergence and the icebergs together, difficulties would still remain. Icebergs sit deep in the water—eight parts beneath the surface to one above—and accordingly keep to the main ocean-currents, which alone can move and carry them. It is, therefore, impossible to see how, with any conceivable submergence, they could reach many of the localities—

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* This uniformity of position, in the case of many of the larger boulders at least, was one of the points the late Dr. Milne-Home, the respected convener of the Boulder Committee, was fond of noticing, and of which he accumulated many examples in his Reports—not perceiving, apparently, how fatal it was to his own theory.

† Milne-Home.—Reports of Boulder Committee.

‡ Dr. A. Geikie states, as the result of many years' exploration, that he had "never succeeded in detecting in the Scottish boulder-clay a single stone which might not have come from rocks not many miles away." ("Scenery of Scotland," 2nd Ed., p. 365.)

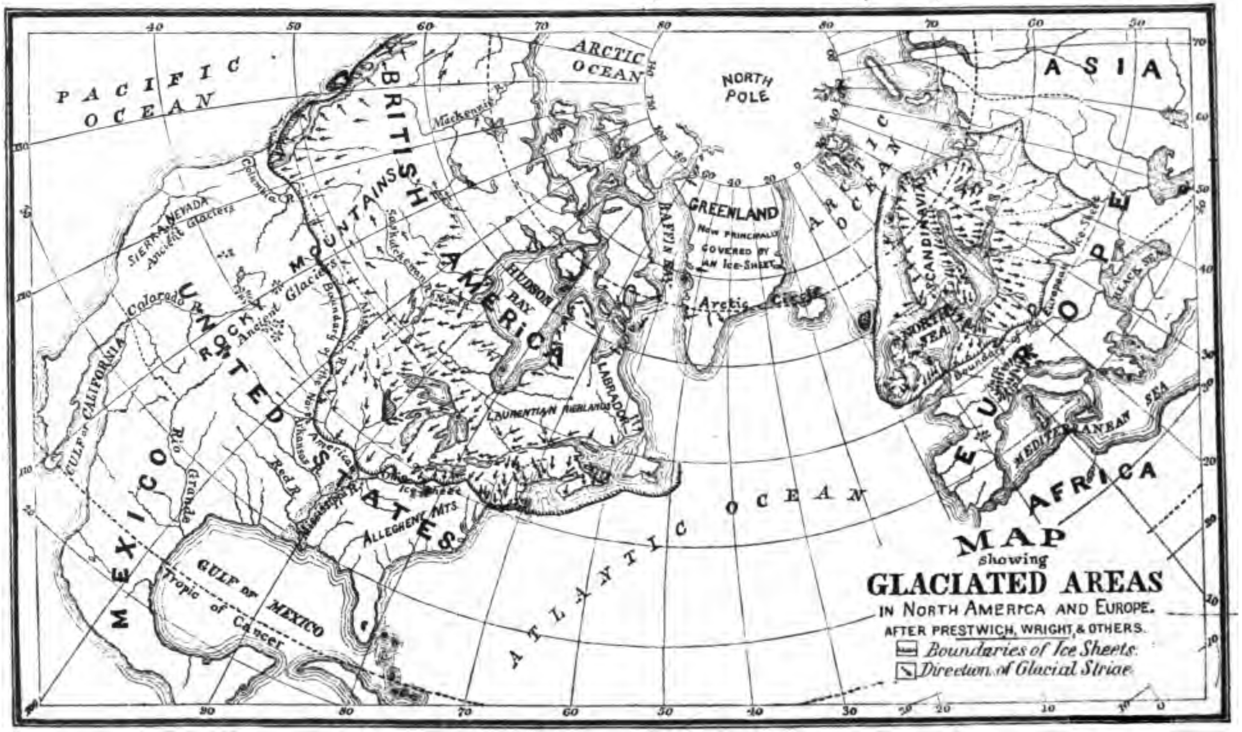
high moors, and glens, and shoulders of mountains—where ice-marks and boulders abound.*

III. The last theory—which it now seems wonderful was so long in being reached—is that of "glaciers" or "land-ice," While the observations and discussions were going on in this country, much attention was being given by geologists on the Continent to the action and effects of glaciers, and to the evidences of their former great extension, particularly in Switzerland, and, indeed, on both sides of the Alps. The names of Venetz, Charpentier, Rendu, Agassiz, Studer, Desor, Martins, and others, will always be remembered in connection with this subject. In 1837, Agassiz announced his belief that Switzerland had at no remote period been deeply buried in ice; that the rocks of all the valleys,

* For example: Boulders and boulder-clay are found on the Sidlaws and Ochils, evidently derived from the Grampians on the other side of the wide valley of Strathmore. Explanation formerly—"icebergs during a period of submergence." But, suppose icebergs during such a period floating off from the flanks of the Grampians (or what remained of them above water); would not the prevailing current--nay, the only imaginable current in the case—carry them *along* the valley of Strathmore, N.E. by Stonehaven, or S.W. by the Forth and Clyde, and never by any possibility *across* the valley, and over the shoulders of the opposite hills?

Again: Dr. Milne-Home found boulders in the upper part of Loch Creran, resembling some of the rocks of Mull, and which he persuaded himself had been brought thither from that island by icebergs. What deep current could possibly turn aside from the line of the Great Glen, opening out straight before it, and flow up a land-locked loch with a group of mountains ahead? There is "no thoroughfare that way." Besides, there is every imaginable proof that the agent which produced the markings on the rocks moved *down* the loch.—Many other instances might be given.

(Of course, it will be understood, we do not question a *moderate* degree of submergence during part of the glacial period,—a few hundred feet probably, corresponding to what has been made out in other countries of the northern hemisphere, and which may have been mainly due to the waters of the sea being subject to an increased attraction towards the pole where the ice was in force as suggested by Jamieson, Croll, and others, and confirmed by Sir W. Thomson. But this is a very different thing from a submergence of 2,000 or 3,000 feet, of which there is no satisfactory proof. Nor do we deny the action of icebergs altogether. Wherever the ice reached the sea, at sufficient depth, of course, icebergs would be given off; but we hold these could only be over the low grounds and along the coasts of the country, and that the effects produced by them, chiefly in the distribution of some boulders, would be comparatively insignificant. Some of these points have been discussed more fully in the *Transactions* of our Geological Society, vols. 8 and 9.)



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up to a great height, owed their grooved and polished surfaces to the passage of this icy sheet over them; and that the transport of huge blocks of rock across the central plain from the Alps to the Jura mountains, was due to the same cause. In 1840 he arrived in this country, and immediately recognised the striated surfaces and boulders that were brought under his notice, as identical with the ice-work which he knew so well in his own land. He visited the Highlands, and parts of England and Ireland, finding everywhere similar phenomena to those which he had been accustomed to observe in connection with the glaciers of Central Europe. His conclusion was that not only did glaciers once exist in the British Isles, but that large sheets of ice covered all the surface, diverging from the main axis of the country, and descending by all the valleys on either side to the sea. But so incredible was this conclusion to geologists generally, that it took more than twenty years to be freely accepted amongst them.*

Gradually, however, it was seen that a great development of land-ice met and explained the phenomena in a way that other theories had failed to do. This key was found to fit the lock, and to turn all the wards of it. The markings and mouldings on the rocks: their universality and persistence from a great height down to the sea-level; their divergence from the principal mountain ranges; the longitudinal striæ on the stones in the clay; the mingling of nearer and more distant kinds among these stones; the clay itself; the erratics and perched blocks, many of them from considerable distances, over various obstacles, and some even from lower levels—were all seen to be more explicable by great sheets of land-ice than by any other known cause. †

Then it is a real and true cause, to be seen in actual operation

* Louis Agassiz, one of the most distinguished naturalists of modern times, was born in Switzerland, 1807; became Professor of Natural History at Neuchatel, 1838; was appointed to the Chair of Zoology and Geology in Cambridge, U.S., 1847, and died there, after a life devoted to scientific research, in 1873.

† Not *one* immense sheet, or "circum-polar ice-cap," streaming out continuously on all sides from the Pole, as some for a time supposed. This is clearly disproved by the ice-marks being locally in various directions, and even turning *northwards* in some places in Europe on the one hand, and north-west America on the other. (See Map, Pl. II.)

The ice-sheets had evidently a number of local mountain-centres from which they radiated in all directions—chiefly, of course, in the direction most open to them in each case.

in many parts of the world at the present day, everywhere producing, in proportion to its size, the very effects under consideration.*

GREAT MASS OF THE ICE.

If now we find ourselves led to this conclusion, we must not refuse to admit the great mass which the ice must have attained to produce the effects assigned to it. This, indeed, is only what might be expected, after what we have learned was the condition of Switzerland during the same period. When the Rhone glacier, for example, with its tributaries, filled the whole length of that extensive valley up to a great height, and stretched continuously

Fig. 4.—*Pierre à Bot,* a granite block from the Mont Blanc range, resting on the Jura (limestone) mountains, about 800 feet above Neuchatel, at a distance of more than fifty miles from its parent rock. Its dimensions are 50 feet in length, 40 in height, and 20 in breadth = 40,000 cubic feet.

* The till or boulder-clay is now generally admitted to be the "ground-moraine" *(moraine profonde)* of the great ice-sheet. But some writers have made a difficulty on this point, thinking that the boulder-clay could not be accumulated under the ice while it extended over the land, but must have been laid down at the point where it reached, or began to float in, the sea. The boulder-clay on the Sidlaws, Ochils, Pentlands, and other detached hills, separated by broad valleys from where the ice had its origin, sufficiently negatives this objection. The error lies in supposing that the ice must everywhere and always cling closely to its channel, and that the water which escapes from it must carry away all the clay that is formed beneath it. There is really no ground for such assumptions.

Our limits preclude us from entering on the various theories of glacier-motion, on which there has been much discussion—the viscous theory, the regelation theory, the molecular theory—nor is this necessary for our present purpose.

outwards across the wide central plain of the country, as a vast *mer de glace* some 3,000 feet in thickness, abutting on the Jura mountains opposite, and even finding its way through some of the passes of these mountains to the neighbourhood of Lyons; when, farther to the east, the glaciers of the Aar, and the Reuss, and the Rhine also attained colossal dimensions; while those to the south of the Alps were on a similar scale, as shown by the huge moraines that rise out of the plains of Piedmont to the height of about 1,500 feet, and the great heaps of *débris* which occur at the lower ends of the Italian lakes. When such was the case, we say, in Switzerland and the north of Italy—all swathed in ice, the plains buried under it, and the mountain valleys brimming over with it from 3,000 up to 7,000 or 8,000 feet in thickness,—what was likely to be the condition of our own country, which is as much farther north from Switzerland as Greenland is from this country?

PARTICULARS.

Accordingly, we need not be surprised to learn that, from all the marks and evidences to which reference has been made, the thickness of the ice in this country must have been very great. On Ben Lomond, for instance, the markings have been traced to a height of fully 2,200 feet, not *descending* the mountain, as if originating there, but passing *across* it, as on its way from a more northern source—doubtless the mountain groups beyond Glenfalloch. The depth of Loch Lomond at that point being 600 feet, this indicates a thickness of ice of nearly 3,000 feet, which corresponds with what has been observed all over the country. All our lochs and arms of the sea—Loch Long, Loch Goil, the Holy Loch, Loch Fyne, Loch Awe, Loch Etive, Loch Creran, Loch Leven, &c.—are simply old glacier channels, which were filled from side to side by the ice, moving outwards from the mountain nucleus of the country to the sea. In Loch Long its volume was so great that part of it pressed over the hills at the head of the Gareloch, and occupied the valley of that loch, marking and moulding the hills on both sides up to their summits. Part of it, again, in like manner passed over into Glen Fruin, uniting with the Loch Lomond glacier in that direction. It completely filled what is now the Firth of Clyde, so that while part of it went southward by the Firth (which, however, was blocked up in great measure by the ice from the Cowals, Loch Fyne, and Arran), a great continuous sheet of it, probably 1,500

to 2,000 feet in thickness, moved eastwards, as its only available outlet, across the midland valley, joined on the one hand by the ice from the upper part of the Forth valley, and on the other by that from the southern uplands, and passing outwards as a broad glacier to the German Ocean.

As observations accumulated, the evidence of these great ice-streams having diverged from the main mountain chains became more and more complete. In the midland valley, for example, as appears from what we have just said, the direction of the striæ and transport is from N.W. to S.E., which led the first observers to suppose that this was the normal direction all over the country. But in Loch Lomond and Loch Long it is from N. to S.; in Loch Fyne and Loch Awe from N.E. to S.W.; in Loch Etive and Loch Leven from E. to W. Along the southern end of the Caledonian Canal the direction is to S.W., but at the northern end it is to N.E., diverging from the mountains in the centre. So also the mountain region of the south of Scotland formed an independent centre of dispersion, the ice, as shown by the striæ and trains of boulders, radiating outwards in all directions: northward, towards Ayr; westward, by Girvan; eastward, by the Nith valley; and southward, by Wigtown, Kirkcudbright, and Dumfries.* (See Map, Pl. I.)

In the N.W. Highlands, so immense was the mass of ice that it passed over the North Minch (28 miles in breadth, but only about 60 fathoms in depth), and overflowed the Long Island to a height of at least 1,250 feet, in a direction from S.E. to N.W.

In like manner, a great body of ice from the southern part of

* One fact is worth noting here—namely, that no boulders from the Highlands are found in the extreme south of Ayrshire, or along the borders of the southern uplands. They are found on the Pentlands and Lammermuirs, which were directly in the way of the great ice-sheet of the midland valley; but "not a single trace of any Highland erratics occurs in those districts of the S.W. and S. which are sprinkled with the grey granite boulders from the Galloway mountains." (J. Geikie, "Great Ice Age," p. 257.) The only reason that can be assigned for this is that the southern uplands were covered by a great ice-sheet of their own, spreading out from them on all sides, so that the Highland ice could not encroach upon them, or carry erratics thither. In short, the erratics belonging to certain groups of mountains are never found beyond the districts known to have been covered by their respective glaciers. This is quite analogous to what has been observed in Switzerland with regard to the separate areas of dispersion of the old glaciers there, and is another convincing proof in favour of land-ice.

the Firth of Clyde, and from the mountains of Galloway, passed into the Solway Firth and the Irish Sea, where, being joined by that from the Cumberland and Welsh mountains on the one hand, and from the Irish mountains on the other, it occupied the whole of the Irish Channel, completely burying the Isle of Man and the Isle of Anglesey, and eroding a deep submarine hollow in the Channel.*

So also the German Ocean appears to have been entirely filled with the ice from Norway and this country, which, coalescing, curved round to the N.W., part of it passing over Caithness, and carrying with it fragments of sea-shells, which it has left included in the boulder-clay of that district.†

OTHER COUNTRIES.

Such was the "Great Winter" in the British Isles. If time permitted, I might show that in Norway the evidences of similar conditions are presented in a still more striking manner, as might be expected from its more northern latitude and the greater height of its mountains. The whole country is marked by ice, which could scarcely be less than 6,000 or 7,000 feet in thickness, and which occupied the entire area of the Baltic Sea, overflowing all its islands, and passing S.E. over Finland into Russia, southward for a considerable distance into Germany, and westward, as has been said, till it coalesced with the ice from this country.

Still extending our view, I might next turn to North America, where evidences of the same conditions are found on the most wonderful scale—undeniable proofs that an enormous sheet of ice

* See Horne on the "Geology of the Isle of Man;" Ramsay on the "Isle of Anglesey" (*Quar. Jour. Geol. Soc.*, vol. 32); and Geikie, "Great Ice Age."

† It is plain that a mass of ice between 2,000 and 3,000 feet thick could not float in any part of our seas, and that it must have dispossessed the sea, so to speak, and moved along the bed of it in the same manner as on the land, till it reached probably as far out as the 100 fathom line, where it terminated in a great sea-cliff facing the Atlantic. (See Croll's "Climate and Time," chap. 27.) Dr. Croll replies to the difficulty some may feel in admitting so great a thickness of the British ice, that it was pressed back and heaped up, so to speak, by the great mass of the Scandinavian ice.

One feature of our glacial deposits can only be mentioned here—namely, the shell-beds found chiefly at low levels along our firths and arms of the sea, containing shells of boreal and arctic species, no longer existing in the British seas but further north, furnishing another striking proof of a great change of climate. (See "Newer Pliocene Geology," by Jas. Smith of Jordanhill.)

extended over the whole North American Continent, from the shores of the Arctic Ocean to the latitude of New York, and even further south, and from the Atlantic Ocean to the Pacific. The "terminal moraine" of this vast ice-sheet has been followed with astonishing continuity across some 3,000 miles of the continent, and the shores of great lakes which were at one time ponded back by it—resembling our own "Parallel Roads," but on a far larger scale—have also been traced.*

General Causes.

Having got so far, we are not yet at the end of our inquiries. The universality of the phenomena all over the northern hemisphere both in Europe and in America, is not to be explained by *local* or *limited* causes (and this of itself is sufficient to negative some of the explanations which have been offered), but indicates rather, as Agassiz first suggested, some *general* or *cosmical* cause.

Climatic Changes.

The interest of this part of the subject is enhanced by keeping in mind that, as shown by the fossil plants found abundantly in Greenland and neighbouring lands, a mild and genial climate prevailed in these far northern latitudes, in times immediately preceding the glacial period. The fossil flora and fauna of our own country also prove that during the early and middle Tertiary periods a warm climate similar to that of the south of Europe prevailed; but towards the close of the Tertiaries there are indications—in the mollusca especially—of a gradual diminution of temperature, and the approach of colder conditions. The question is thus opened up as to the cause of these great changes of climate.

Early Theories.

Here, also, we have to notice various theories that were propounded, only after a short time to be discarded. First, a *change*

* The marks of the glacial period in the Northern States of America have been wrought out by the geologists of the U.S. Survey in the most exact and thorough style; and geologists all over the world are indebted to the liberality of the U.S. Government in placing the handsome volumes in which the labours of the Survey are recorded so freely at their disposal.

An admirable volume on "The Ice Age in North America" has recently been published by Professor G. F. Wright, LL.D., Assistant on the Survey. [London: Kegan Paul, Trench, & Co., 1890.]

in the earth's axis was supposed, bringing the pole far down into the temperate zone. This may be dismissed, for astronomers say it is impossible, and that the axis remains at all times precisely, or with very slight variation, the same.* Next, a *variation in the sun's heat* was suggested; that the sun is a "variable star," dying down and kindling up again from time to time. Or again, that both the sun and earth pass at intervals through *colder tracts of space.* These may also be dismissed as mere conjectures of things unknown. Then a more real cause, one more within observation, was adduced by Sir Charles Lyell many years ago, in the *varying distribution of sea and land.* He pointed out that a preponderance at any time of land at the equator and sea at the poles, would cause a warm climate; and, on the other hand, a preponderance of sea at the equator and land at the poles, would cause a cold climate, over what we now call the temperate zones.† It is certain that the sea is a great equaliser of climate, and that when the land is much broken up into islands and groups of islands, the climate is less extreme either way, as to heat or cold, than when there are great masses of land, and some of it high land, at a distance from the sea. And some writers have argued that such geographical changes, by altering the bed of the Atlantic Ocean and the configuration of the American continent, would probably deprive us of the Gulf Stream, to which we are mainly indebted for our mild climate in this part of the world. It is estimated that our average temperature is about 15° better than otherwise it should be, owing to this warm ocean current. With this current cut off or turned in another direction, and with a mass of high land extending to the north, there can be little doubt we should have the conditions of a glacial period.

Gradually, however, sundry objections began to be made to this "geographical" theory. It was pointed out that the phenomena were too general to be accounted for in this way; besides, that there was no evidence of such great changes in the distribution of

* Professor G. H. Darwin, in an elaborate paper on "The Influence of Geographical Changes on the Earth's Axis of Rotation" (*Phil. Trans. Roy. Soc.*), concludes that a deflection of the axis to the extent of 1°, or not more than 3°, *might* be due to such changes; but apparently even this would require such extensive deformations of the earth's crust, and alterations in the distribution of sea and land, as geologists must hesitate to admit, especially within comparatively recent periods.

† "Principles of Geology," vol. I., p. 260.

sea and land within the period referred to; and that, in fact, it would rather seem that, within very small, one might almost say minute oscillations, the distribution of land during the glacial period, and long before, was much the same as at present. From facts that have recently been brought to light by deep-sea soundings, many geologists believe that the great ocean areas have remained much the same from a very remote geological period; and that the changes which have occurred in parts, chiefly along their margins, are not such as would in themselves produce any appreciable effect on the climate of the neighbouring lands.*

It was thus felt that changes in physical geography, though undoubtedly true and effective to a certain extent, were inadequate to account for such great revolutions in climate as seem to have taken place all over the northern hemisphere. Another explanation was therefore still sought for.

Astronomical.

The question came to be: Apart from those which had been discarded could there be any causes external to the earth—any *astronomical* causes—of such changes of climate? That which seemed most likely to offer a solution of the problem was the varying *eccentricity of the earth's orbit.* So long ago as 1832, Sir John Herschel had referred to this as bearing upon climate, but he thought it unlikely to produce any great climatic change, because the diminution in the heat received by one hemisphere or the other, during a period of greater distance from the sun, would be compensated for by the greater length of the season, so that on the whole year both hemispheres would receive equal amounts of heat. About twenty years ago, however, the late Dr. James Croll, of the Geological Survey of Scotland—a remarkable man, and very able reasoner,—took up the subject, and in a series of elaborate papers first published in the *Philosophical Magazine*, and since embodied in his great work "Climate and Time," † showed that, notwithstanding the truth of Sir John Herschel's general statement, a high eccentricity of the earth's orbit would still lead to a great extension, in both hemispheres alternately, of glacial conditions.

* See Sir William Dawson's Address to the British Association, 1886, and Dr. A. Geikie's "Text-Book of Geology," 2nd Ed., pp. 36, 272, 600.

† London: Isbister & Co., 1875.

CROLL'S THEORY.

Now I must assume that this theory—"Croll's Theory," as it is usually called—is, at least in its outlines, well-known. Some readers, however, may be reminded that at present the eccentricity of the earth's orbit is about three millions of miles, and that our winter occurring in *perihelion*, or when nearest the sun (because we are then leaning away from him), is about eight days shorter than our summer. In the southern hemisphere, of course, the case is reversed, the summer is shorter than the winter. But, owing to what is called the "precession of the equinoxes," a time comes round every 10,500 years when the positions in this respect are changed, and winter in the northern hemisphere occurring in *aphelion*, or when farthest from the sun, is longer than the summer. Not only so, but owing to the slowly-varying ellipticity of the earth's orbit, increasing for long ages and then gradually diminishing, periods occur at certain long intervals when the eccentricity may be, not three millions merely, but as much as 13 or 14 millions of miles. The winters would then be about 36 days longer than the summers, and correspondingly more severe. The short summers would be insufficient to melt the accumulated ice and snow of the long winters. And these conditions would be accompanied by such changes in the prevailing winds, in the ocean currents, and in the precipitation of vapour, as would be amply sufficient, in Dr. Croll's view, to account for all the phenomena of a glacial period.

The last period of great eccentricity is calculated to have begun about 240,000 years ago, and to have lasted till 80,000 years ago. This is when the last glacial period—that referred to when we use the term—is supposed to have happened. Similar periods are calculated to have occurred 750,000, 850,000, 2,500,000, and 2,600,000 years ago.*

OBJECTIONS.

Such is an outline of Dr. Croll's celebrated theory, which may be said to have held the field for about twenty years. Quite recently, however, sundry objections have been advanced against it.

(1) One consequence of it clearly is that *many* glacial periods must have occurred in the earth's history, and that evidences of

* The orbit made its nearest approach to a circular form 50,000 years ago, and is now tending to greater ellipticity again. But it is so far reassuring to learn that there will not be another period of "maximum eccentricity" till after 450,000 years. ("Climate and Time," p. 312, &c.)

them might be expected to be found among the earlier formations. There are certainly conglomerates and breccias in the Silurian, Old Red Sandstone, lower Carboniferous, Permian, Jurassic, and Cretaceous formations, as well as in the early Tertiary strata, which some geologists have been disposed to regard as indicating the recurrence of glacial conditions in all these periods. But these, even if admitted, do not seem to be sufficient. As Dr. Prestwich has pointed out, in the 100 million years of geological time, which Dr. Croll adopted from Sir William Thomson, there should have been, not eight or ten merely, but 130 to 160 cold periods;* and, even of these eight or ten, the majority are considered doubtful. Dr. Geikie, in his "Text-Book," mentions only two with confidence, and other two with doubt; and, in the two that are accepted, the phenomena appear to indicate *local*, and not *general*, glaciation.† So that as Professor Le Conte remarks: "Of the recurrence of many glacial epochs in the history of the earth, there is as yet no reliable evidence. It is true that what seem to be glacial drifts with scored boulders, &c., have been found on several geological horizons, but these are usually in the vicinity of lofty mountains, and are probably, therefore, evidence of *local* glaciation, not of a *glacial epoch*. On the other hand, all the evidence derived from fossils plainly indicates warm climates even in polar regions during all geological periods until the quaternary. The evidence at present, therefore, is overwhelmingly in favour of the *uniqueness* of the glacial epoch. This fact," the writer adds, "is the great objection to Croll's theory."‡ A distinguished French author, M. Falsan, in his very clear and able work, "La Période Glaciare," writes to the same effect:—"For ourselves, there is nothing less proved than the periodicity of glacial phenomena." He points out that those who have given themselves to the study of Fossil Botany in all the earlier formations, have found no trace of such recurring cold periods; and he concludes that the existence of glacial periods before the Miocene, in the Secondary and Palæozoic formations, is quite inadmissable, and contrary to all the facts of Palæontology.§

(2) Another consequence of the theory is that several intervals of more genial climate would occur during what we should call, as a whole, the glacial period. As already stated, owing to the

* "Geology," vol. II., p. 528.

† Mr. G. K. Gilbert in *Nature*, 1883.

‡ "Elements of Geology," p. 577. § *Op. cit.*, pp. 196, 218.

precession of the equinoxes, the seasons in the northern and southern hemispheres alternate every 10,500 years; and as a period of high ellipticity lasts 160,000 years or more, there must, during that long period, have been many such alternations. That is to say, our hemisphere, during the long cycle of great eccentricity, would experience several glacial periods, and several periods of genial climate—periods of long, mild summers, and periods of continuous arctic winter—each lasting for many thousands of years. Some of our leading geologists believe they can trace these mild interglacial periods in the drifts and shell-beds of the epoch as a whole; but others are more doubtful, and do not think this has been made out as clearly as, according to the theory, might be expected. Mr. Gilbert remarks—"If the hypothesis is true, the cold of the glacial epoch must have been many times interrupted by intervals of exceptional warmth; but little has been added to the evidence for such an interruption, and in America, where there is now great activity in the investigation of glacial phenomena, the evidence of a *single* interglacial period is cumulative and overwhelming, while there is no indication whatever of more than one."*

Dr. Prestwich, also, while admitting minor vicissitudes of climate, and a partial retreat of the ice at times, followed by a renewed advance, does not admit "interglacial periods" as usually understood.† And M. Falsan, to whom I have already referred, is quite opposed to two, or more, distinct glaciations; maintaining the unity of the glacial period, and pointing out that a simple oscillation of the ancient glaciers, similar to what we see existing glaciers are subject to, is sufficient to account for the "two boulder-clays" which some geologists make so much of, and which he calls "a simple local accident."‡ So that here, also, there is considerable diversity of judgment.

(3) Another point is connected with the *date* or antiquity of the glacial period. According to Croll's theory, as I have said, it began about 240,000 and ended about 80,000 years ago. But of late, doubts have been accumulating with regard to such an antiquity being assigned to it. The freshness of the markings on the rocks; the rounded contour, still, of the knolls and eminences; the little waste, apparently, that has taken place since it passed

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* *Nature*, vol. 27.

† "Geology," vol. II., p. 459.

‡ "La Période Glaciare," p. 213.

away, as tested by the position of perched boulders, and the rocky surfaces on which they very often rest, compared with the surrounding rocks — all seem inconsistent with the lapse of so many tens of thousands of years as that theory requires. "The present tendency," as Professor Wright, of America, remarks, "both among geologists and astronomers, is to diminish estimates of time in almost every period." And the same writer points out that the changes in species since the Ice Age have been almost inappreciable. "The flora and fauna of the world during the glacial period were essentially the same as those of the present time. Even man is believed to have been an inhabitant of America, as well as of Europe, before the ice had withdrawn. If these changes in the organic world have been so slight since the glacial epoch, it follows that the farther back that period is placed in time, the greater are the difficulties of the evolutionists."* For, if little or no change has taken place in 200,000 or 240,000 years, what ages must be required for the evolution of all the different orders of organic life, of which the rocky tablets of the past bear witness ! †

OTHER RESEARCHES.

Considering, then, that the astronomical theory of the glacial period is in all these respects open to question, the American geologists have of late been giving their attention to the more direct geological evidence bearing on this point. This they find mainly in two classes of facts—namely, (*a*) the amount of erosion and disintegration which has occurred since the glacial period, and (*b*) the extent to which lakes, peat-mosses, and "kettle-holes," have been filled up since that period. Now, Prof. Winchell has gone carefully into the rate of erosion of the Falls of St. Anthony on the Mississipi; Mr. Gilbert has made fresh and minute observations and calculations regarding the recession of the Falls of Niagara (which he finds Sir C. Lyell and others very much *under*-estimated); Dr. Andrews has taken the rate of erosion of the shores of Lake Michigan and the resulting accumulation of beach sand, in dunes, at the south end of the lake; and Professor Wright has based his

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* "The Ice Age in North America," p. 450.

† Dr. Prestwich, in England, gave expression to the same doubts and objections several years ago. (*Quar. Jour. Geol. Soc.*, vol. 43.) In his "Geology," more recently published, he sums up by saying—"It seems to me that the shorter estimate of time is the one more in accordance with all the conditions of the problem." (Vol. II., p. 534.)

calculations on the filling upof small lakes and peat-bogs surrounded by kames, at Andover, Mass. All these computations of post-glacial time, reached independently, closely agree in pointing to a period of some 8,000 or 10,000 years only, as having passed since the ice-sheet of the glacial epoch melted away. "It is therefore impossible," says Mr. Warren Upham, "to refer that glaciation to an epoch of increased eccentricity which ended 80,000 years ago."

But if we abandon Croll's theory, what do we fall back upon as the cause of the glacial period? Here, at this moment, much uncertainty prevails. Some American geologists have recently been looking again in the direction of geographical changes, and thinking that there may have been in North America, and possibly in other parts of the northern hemisphere, a considerable elevation of the land above its present level, at the *beginning* of the glacial period, which would set it agoing, so to speak. Some facts in connection with old river channels seem to favour this conclusion; and perhaps, other causes concurring, no great difference in this respect would be sufficient. But when 1,000 feet or more of additional elevation are predicated for the whole Continent of North America, and perhaps also for all northern Europe, I think we should be chary in having recourse, without good evidence, to such great disturbances of the earth's crust, at such a recent period. In this, we should humbly follow the great naturalist, Darwin, who frequently expressed his dissent from those theorists who were always ready to "raise or depress Mother Earth" to suit their own views, and said he could not bring himself to admit such immense geographical changes—whole continents being raised and lowered again—within the life-time of existing species.*

PERHAPS NO GREAT CHANGE REQUIRED.

"The sum of the whole matter," says Professor Wright, "is, that as far as theory is concerned, we do not as yet know what was the ultimate cause of the glacial period. But," he adds, "there is no lack of seemingly adequate causes. A great many things may have produced the glacial period. There is, for all we know to the contrary, always moisture enough in the air, and instability enough in the crust of the earth, to make a glacial period imminent.

* "Life," vol. II., pp. 38, 58, &c.

A little change in the direction of the aërial currents may readily be conceived to start a period of glaciation." *

In this connection it may be pointed out that it is not intense cold that is solely or chiefly required to produce a period of glaciation. Cold itself is powerless to nourish the glaciers; there must be a copious supply of vapour, as well as an area of precipitation. Professor Tyndall, it will be remembered, sums up the matter thus:—"Without solar fire, no atmospheric vapour; without vapour, no clouds; without clouds, no snow; without snow, no glaciers." So that "the cold ice of the Alps has its origin in the heat of the sun."†

Indeed, there seems reason for the conclusion that a very slight diminution of the mean annual temperature in Britain and Switzerland would suffice, in either of these countries, to bring the Great Winter back again.‡

Nay, in Switzerland it lingers still on all the higher grounds, advancing or retreating according to the degree of humidity in the prevailing winds, and consequent precipitation of snow upon the mountain-ranges. (Fig. 5.)

Fig. 5.—Alpine Summits.

General View: Conclusion.

Again—and this is the last piece of theory I have to mention,—some eminent Continental geologists have lately adopted another, and, perhaps, wider, view of the whole subject. They consider the glacial period to be the natural result of the gradual cooling and

* "Ice Age in North America," p. 441.

† "Forms of Water," p. 7.—Consequently "cold tracts of space," or a "diminution of the heat of the sun," or any general lowering of temperature, are not the causes required.

‡ Falsan: "La Période Glaciare," p. 199.—T.G. Bonney, *Nature*, 1891.

condensation both of the sun and the earth. They believe that for long ages, owing to the greater diameter of the sun, and consequent greater nearness to the earth, and owing also to the slowly-dissipating heat of the earth itself, there was great uniformity of climate—a warm and humid climate—prevailing through all the earlier formations, in all parts of the world. They consider it to be useless, therefore, to look for evidences of glacial periods in the earlier formations; for then the climates were not sufficiently differentiated. But the gradual concentration of the solar nebula, and the constant diminution of the heat of the earth itself—both of which are now regarded as among the certain data of science—brought about, in course of time, a condition of "unstable equilibrium" in the climatic conditions, till at length a moment came when snow and ice made their first appearance on the earth. And that was the beginning of the glacial period, in one of the many stages or phases of which we are living at this day.

I will merely remark that, though this may be accepted as true in a general sense, it does not seem to account for the great temporary (though sometimes long-continued) changes of climate, which can be shown to have occurred in, at least, the later geological ages, and in which we include the glacial period. These seem to require some more special cause—either that on which Croll bestowed so much labour, or one that still awaits discovery.

The subject, like every other department of Nature, how insignificant soever it may at first sight appear, opens up, the more we study it, an ever-widening range of inquiry, and discloses the wonderful extent and complexity of that great system of Natural Order in which we are placed.

XVIII.—*Giphantie: A remarkable forecast of Photography in 1760.* By WILLIAM LANG, jun., F.C.S., President of the Glasgow Photographic Association.

[Read before the Society, 17th December, 1890.]

THOSE interested in photography and its literature have known that a sort of foreshadowing of the art had been given out many years before photography became an accomplished fact. The source, however, whence this anticipating of events arose was not by any means very clearly established. It is now traceable to a French writer who flourished during the latter part of last century; and as I was fortunate enough last summer to secure a copy of the work wherein the prophecy, if one may thus put it, is to be found, it may interest the members of the Glasgow Philosophical Society to have some details regarding the book and its author. The volume I have brought with me, and is here for your inspection. The title of the book is "Giphantie," a French word, the equivalent of which in English may be rendered as Giphantia. It may be here explained that this title is the name of an island in the centre of Africa, which the author supposes to have been given to supernatural beings one day before Paradise was closed on the ejection of Adam. The author of the work was one Charles François Tiphaigne de la Roche, a doctor of medicine, and a man of literary talents. Most of his writings are of a distinctly idealistic nature. He wrote a few scientific works—one of them, entitled "Physical Observations on Agriculture, Plants, and Minerals," was published in 1765. Giphantie is, in reality, a coined word, being an anagram of Tiphaigne. The edition which I have here this evening bears the date of 1760, and purports to have been published at Babylon. As far as I can learn, this seems to have been the first edition, and it is published anonymously. Another French edition was issued in 1761, but this was published at La Haye by Daniel Monnier. There is also an English translation, the title of which is—"Giphantia: or a view of what has passed, what is now passing,

and during the present century what will pass, in the world. Translated from the original French, with explanatory notes. London: Printed for Robert Horsfield, in Ludgate Street, 1761." It seems another edition was published in Cherbourg. That the book is now very scarce there can be hardly any doubt; the British Museum has three copies, different editions.

The copy I have with me is made up of two parts, the first containing twenty chapters, the second sixteen. I need not go into the names ascribed to each. The one which interests us more especially is Chapter XVIII. of Part I., entitled "The Tempest." It is not necessary to translate it in full, but only that portion which bears more immediately on the matter which I wish to bring before your notice. It may be explained that the author, having been led by his spiritual guide into a darkened chamber, sees there what is thus described:—

"I saw out of a window a sea which seemed to me to be about a quarter of a mile distant. The air, full of clouds, transmitted only that pale light which forbodes a storm, the raging sea ran mountains high, and the shore was whitened with the foam of the billows which broke on the beach.

"'By what miracle,' said I, to myself, 'has the air, serene a moment ago, been so suddenly obscured? By what miracle do I see the ocean in the centre of Africa?' Upon saying these words I hastily ran to convince my eyes of so improbable a thing. But in trying to put my head out of the window I knocked it against something that felt like a wall. Stunned with the blow, and still more with so many mysteries, I drew back a few paces.

"'Thy hurry,' said the Prefect, 'occasions thy mistake. That window, that vast horizon, those black clouds, that raging sea, are all but a picture.'

"From one astonishment I fell into another. I drew near with fresh haste, my eyes were still deceived, and my hand could scarcely convince me that a picture could have caused such an illusion.

"'The elementary spirits,' continued the Prefect, 'are not so clever painters as they are adroit philosophers thou shalt judge by their manner of working. Thou knowest that the rays of light reflected from different bodies make a picture, and paint the bodies upon all polished surfaces, on the retina of the eye for instance, on water, on glass. The elementary spirits have studied to fix these transient images, they have composed a most subtle matter, very viscous and prompt to harden and dry, by the help of which

a picture is made in the twinkle of an eye. They do over with this matter a piece of canvas, and hold it before the objects they have a mind to paint. The first effect of the canvas is that of a mirror. There are seen upon it all the bodies, far and near, whose image the light can transmit. But what the glass cannot do, the canvas, by means of the viscous matter, retains the images. The mirror shows the objects exactly, but keeps none. Our canvasses show them with the same exactness, and retain them all. The impression of the images is made the first instant they are received on the canvas, which is immediately carried away into some dark place. An hour after, the subtle matter dries, and you have a picture so much the more valuable as it cannot be imitated by art or destroyed by time.'"

So ran the day dream of this fantastic writer Tiphaigne; but surely all the wondrous properties of the canvasses, coated by the elementary spirits, have had their fulfilment in the photographic plate of our own time, and, curiously enough, the substance which plays an important part in the preparation of the sensitive coating of the plate could scarcely be more appropriately described than as "very viscous and prompt to harden." I refer, of course, to the gelatine which holds the sensitive silver salt in situ on the plate. I think it must be admitted that what Tiphaigne wrote in 1760, read now in the light of what has been accomplished by means of photography, is, to say the least, somewhat remarkable. There are several other very curious things to be found in Giphantie. A chapter, the 9th of the first part, entitled, "The Conversations," deals with what may be considered the wonders of the Telephone. Chapter XVII. of the second part, and concluding chapter of the work, treats of a subterranean passage. It is a description of a river underground, and when we remember that the scene is laid in Africa, and that Rider Haggard has made use of a similar idea in his novel "Allan Quatermain," the coincidence is most striking that two imaginative writers, one flourishing in last century and one of our own time, should, as it were, work out one and the same idea.

XIX.—*The Optical Lantern in Class-Room Work.* By ARCHIBALD BARR, D.Sc., M.Inst.C.E., Regius Professor of Engineering in the University of Glasgow.

[Summary of Paper read before the Society, 18th March, 1891.]

THE Optical Lantern has been extensively adopted for the illustration of popular and public lectures, but it has not yet been very generally applied in ordinary class-room work. This has arisen, no doubt, from a variety of circumstances attending its use, the principal of which are probably these:—(1) The necessity for darkening the lecture room in order to make the lantern pictures visible; (2) the transient character of the pictures, which renders them unsuitable in some cases for class-room use; (3) the necessity which usually exists for employing an assistant to work the lantern, and the consequent difficulty of referring back to an illustration, as it is often desirable to do in class-teaching; (4) lanterns in every way suitable for class-room use have not been available, at least until lately; (5) supplies of oxygen for the production of the lime light were not conveniently obtainable until quite recently; and, (6) the production of lantern slides in sufficient quantities to illustrate every detail of a subject, has been a matter of some difficulty and no little expense.

Arrangements made at the Yorkshire College, Leeds, to overcome these difficulties, have led to a very great extension of lantern illustrations in ordinary class work, and it appeared to the author that a brief description of some of these arrangements might be of interest to many members of the Society, and especially to those engaged in teaching.

The first drawback to the use of the lantern, namely, the necessity for darkening the lecture-room when exhibiting the projected pictures, may very simply be overcome, so much so that during the day-time the room need only be very partially darkened, while in the evening, when the light is less diffused, no darkening whatever is necessary. In the Engineering Lecture-Room at Leeds, for example, the projections are shown while sixteen incandescent lamps are lighting up the room quite brightly. The

essential condition is simply that the lantern screen shall be in shadow, when it will be found that an ordinary lime-light lantern will give excellent results though the room, generally, may in the day-time be moderately lighted and in the evening may be fully lighted.

Briefly, the arrangement is this:—The lantern screen, which is placed over the blackboard, is inclined at such an angle, that, while the electric lamps which are situated near the ceiling illuminate the lecture table, the blackboard, and any diagrams hung on the walls, no direct light falls upon the screen. By this arrangement lantern pictures, ordinary diagrams, blackboard notes, and models, or experiments can be shown simultaneously.

The transient character of the illustrations evidently cannot be overcome, and where it is essential that an illustration should remain continuously before a class for a long period, ordinary diagrams must be used. Such cases are, however, very few, because usually the student may have time enough allowed him to sketch such simple illustrations as it is advisable to use in ordinary class work, and if not, he can be referred to some book or journal in which he may find a similar illustration which he may study again at leisure. The transient character of the illustration is, however, by no means altogether a drawback. Only one illustration being shown at a time, the student's attention is not distracted by his desire to copy illustrations not at the moment being referred to by the lecturer.

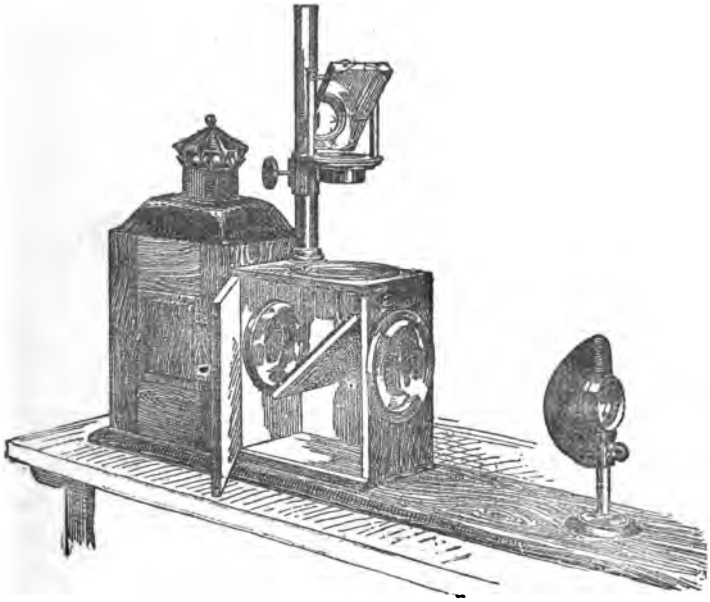
The lantern is of little value for ordinary class work, if it is necessary to employ an assistant to work it. By using a short focus lens, however, the lantern may very conveniently be placed on the lecture table, and the slides laid out upon a sheet of white paper, so as to be readily seen by the lecturer in the lighted room, and any one may be shown as frequently as may be necessary during a lecture.

For ordinary slide work many suitable lanterns are now in the market, and require no description. A suitable lantern for the exhibition of slides *and experiments* has been a great desideratum, especially for the illustration of lectures on physics, chemistry, physiology, &c., but the want appears to be very fully met by the lantern now exhibited.

This lantern has been devised by Professor Stroud and the Rev. Mr. Rendell to meet the requirements of lecturers who desire to exhibit both slides and experiments; but it possesses special

advantages for the class-room, even when experiments have not to be projected on the screen.

As will be seen in the illustration, the lantern is arranged for either horizontal or vertical projection. The sloping mirror, shown in the front part of the lantern, receives the beam from the first lens of the condenser, and reflects it vertically through the second lens, fixed horizontally in the top of the front portion of the lantern, and thence through the objective, which can be moved



up and down for focussing by means of a rack and pinion on the pillar. The mirror fixed above the lens projects the beam at any required inclination towards the screen. The lower mirror is hinged at its upper edge to the front board of the lantern, and, by means of a stud projecting through the curved slot in the side of the box, it can be moved upwards so as to permit the beam to pass horizontally through the condensing lens seen in the front of the box, and then through the objective, seen on the extreme right of the figure.

Some pieces of experimental apparatus require, from their nature, to be placed horizontally, and others vertically. Apparatus of the former kind is placed on the top of the box, whereas apparatus which requires to stand vertically is placed in front,

and the hinged mirror can be instantly altered in position to exhibit either, at will, on the same screen.

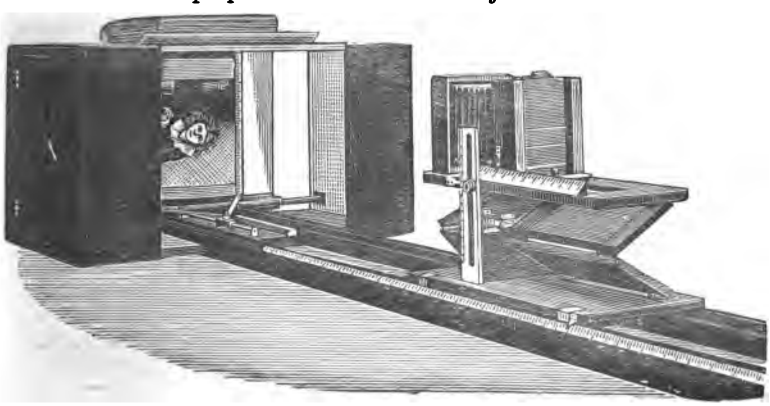
This lantern can be used for the exhibition of slides in the ordinary way, or the slides may be simply placed in a frame, laid upon the condensing lens on the top of the box. The latter arrangement has the great advantage that, if the lecturer, facing his audience, stands in front of the lantern and has the screen behind him, he sees the illuminated lantern slide right way up, so that he can read any printing or reference letters upon it, and can point with a pencil or needle to any portion of the slide. The awkwardness of the lecturer requiring to turn his back upon his audience and use a large pointer is thus obviated, and he is ready at any moment to substitute one illustration for another. Further, the upper surface of the slide being unobstructed, a square of finely ground glass, rendered transparent by paraffin or other oil, may be placed in the lantern, and, using a hard pencil, sketches or writing can be produced on the screen before the audience, and any portion of the writing or sketching is readily rubbed out by means of a pad moistened with paraffin. In this way all the advantages of the blackboard, in facilitating the development of illustrations before the class, are obtained, and besides, when the screen has been filled the matter need not be rubbed out, but may be laid aside, to be again exhibited at any time. Diagrams may, of course, be rapidly prepared before a lecture, and any portions may be omitted and supplied before the audience. Square and compasses can be readily used in the lantern. Admirable lantern slides can be produced upon clear glass by means of a pen and stencil ink, and the ink can also be used for making temporary additions to ordinary photographed slides by sketching upon the cover glass. The principal source of lantern slides, however, will usually be illustrations taken from books, periodicals, or natural objects reproduced by photography.

The difficulties met with in photographing book illustrations, engravings, &c., by means of an ordinary camera, are chiefly these:—(1) the supporting of the book so that the illustration to be photographed shall be properly presented and held flat; (2) the even illumination of the illustration; (3) the adjustment of the camera, at the proper distance from the illustration, to secure a photograph of the desired size; (4) the adjustment of the camera so as to have the image in the centre of the plate, and to

have the lens directed perpendicularly to the illustration at its central point. These difficulties are, of course, not insurmountable when an ordinary outfit is used, but they are of such a troublesome kind—on account of the adjustment being disturbed in one respect while being improved in another—that satisfactory results are seldom obtained, and, as a consequence, the preparation of slides from book illustrations, engravings, &c., is expensive and little practised. The fourth adjustment referred to is especially troublesome with ordinary apparatus, and any want of accuracy in its accomplishment causes the photograph to be a perspective of the original instead of a true copy, lines which are parallel to each other in the original being inclined in the photograph.

An apparatus was exhibited for overcoming these difficulties, consisting, in the first place, of a "book-holder"—seen to the left in the illustration—in the form of a box-shaped frame, partly open in front, and provided at the back with two vertical sliding boards—one of which is seen in the figure. These boards are so connected together that they are constrained to move simultaneously, and to an equal extent, towards or from the centre of the frame. A scale is provided upon the frame, to indicate the width of the space between the inner edges of the boards. The edges of the boards are furnished with vertical scales, to be used in adjusting the camera to the required height. A pair of Argand gas burners or paraffin lamps are provided—one behind each of the front boards—to illuminate the illustration.

The camera support slides upon a railway, which is attached to the book-holder by pins, and can be lifted off and folded up when not in use. The railway carries a scale, used for setting the camera at the proper distance from the object.



The carriage, which slides on this railway, supports the camera by means of two pairs of hinged boards, the hinged edges of one pair being at right angles to those of the other pair. This novel mechanism allows the camera to be raised and lowered only in a vertical line, and prevents it going out of level, either longitudinally or transversely. A spring is fixed at the hinge of one of the pairs of boards, so as to force them apart and very approximately balance the camera, which can be raised and lowered by a touch, and will not fall when let go. A vertical slotted rod with binding screw serves to fix the camera at any required height. This slotted rod has a scale upon its edge, to enable the operator to set the lens level with the centre of the picture.* The back end of the camera slides upon the base (no screw is used), and a scale is provided for setting it to the correct focus.

The method of using the apparatus is as follows:—The object or illustration to be photographed is measured. Suppose it to be a book illustration, and to measure 6 inches in the direction of the height of the page, and 8 inches in the direction of the breadth of the page. The sliding boards of the book-holder are opened to 6 inches, as indicated by the scale on the frame, and the book is placed, as shown in the engraving, with the page opposite the illustration resting on the top of the book-holder and the illustration itself exposed between the edges of the sliding boards. If the book is thus held open, the illustration is flat and vertical and its centre is exactly over the centre line of the railway, and the carriage is now moved along the railway till the index points to 8 (8 inches being the longer dimension of the illustration assumed), when the camera is at the correct distance from the illustration to give the largest photograph that can be put on a slide of the ordinary size. The back of the camera is now moved to the graduation 8 on the base, and it will then be perfectly focussed—as exactly focussed as it could be by means of the ground glass after very careful and slow adjustment. The operator next reads from the scale on one of the sliding boards of the book-holder, the graduations opposite the top and bottom edges of the illustration—let these be 1 and 9 respectively. He adds these together, and sets the index attached to the camera base to the sum (10) upon the scale on the vertical slotted rod,

* Since designing this mechanism, the author has learned that a mechanism similar in principle had previously been devised by Mr. H. M. Brunel for other purposes.

and clamps the camera at this level. The lens is now at the height of the centre of the illustration.

In this way, without using any ground glass, the camera is set at once, so that it will give a *true* copy of the original, exactly of the required size, exactly in the centre of the plate, and perfectly in focus. All the adjustments are easily made in a few seconds. The dark slide is dropped in, the shutter lifted, and the lights turned up simultaneously by one stop-cock. No cap need be used. The exposure will depend, of course, upon the kind of plate used, the size of the stop (if any), and the nature of the light: 1 minute or 1½ minutes will usually suffice. Transparencies are printed in the usual way from the negatives obtained—most conveniently by gas or candle light. Loose engravings, sketches, &c., are held in position by a large book placed in the manner described, or they may be pinned on to a board having a projecting ledge which rests upon the top of the book-holder.

To prepare transparencies directly from negatives of any size, the negatives are supported at the front of the book-holder by means of a pair of grooved carriers, and a sheet of white paper is placed at the back of the book-holder to reflect the light through the negative. The scales are used as before, except that a mark on the carriage, near the lens end, is used as the index for the railway scale, and the vertical scale on the edge of the front board of the book-holder is used instead of the scales on the sliding boards.

Slides may be prepared in a similar manner from natural objects, such as botanical or other specimens, which are placed at the back of the book-holder, and illuminated by one or both of the lights, as may be required, to show them in proper relief.

The appliances described have been in use for some time at the Yorkshire College, Leeds, and have led to a very great extension in the use of the lantern in almost all the classes—for example, in chemistry, physics, biology, engineering, English history, classical antiquities, and in the technological and medical departments. Many thousands of lantern slides have been prepared in this way, and a photographic operator is constantly employed to meet the demand for fresh illustrations.

The substitution of lantern illustrations for wall diagrams has the great advantage that one moderately-skilled photographic operator can, at a cost of a few pence per slide, make perfect reproductions of illustrations bearing on subjects so widely differing

as biology and engineering; and a dozen illustrations can easily be ready at a day or two's notice. On the other hand, to prepare large diagrams requires, in most cases, very considerable artistic skill and a more or less intimate acquaintance with the subject matter of the illustration, and besides, the process is a very slow and costly one. No doubt the time will soon come when the lantern will be extensively used, not only in universities and colleges, but in ordinary day schools, in connection with all subjects capable of pictorial illustration.

XX.—*Remarks on the First Edition of the Chemical Writings of Democritus and Synesius.* Part II. By Professor JOHN FERGUSON, LL.D., F.R.S.E., F.S.A., F.S.A.Scot.

[Read before the Society, 19th November, 1890.]

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1. The paper on this subject which I communicated to the Society on Nov. 19, 1884, exactly six years ago, was occasioned by my having seen an edition of Democritus, dated a year earlier than that of Padua, 1573, which is considered by Dr. Kopp, the chief authority on the subject, as the first. This edition, published at Cologne by Johann Birckmann in 1572, appended to Mizaldus' *Memorabilia*, of which I had discovered copies in the Hunterian and University Libraries here, was fully described in the paper, and it was also pointed out that the existence of it necessarily involved important modifications in the opinions advanced as to the date and place of the first publication of the book.

In the same paper there was described another edition, dated 1573, also published at Cologne by Johann Birckmann, a copy of which I had found in the British Museum. This was new, for no previous writer about Democritus, so far as I am aware, makes any allusion to it whatsoever. It is a reprint of the previous edition, and is exactly like it in size, type, and arrangement, so much so that one might have supposed it to be an issue of remainder copies with a re-dated title-page, but a cursory examination showed that the two editions were typographically different from end to end, and that the 1573 is a veritably new edition.

2. The Cologne 1572 edition just mentioned was not absolutely

unknown. It had been referred to by Fabricius and Lambeccius,* but in so vague a way that Dr. Kopp, who knew only the Padua edition of 1573, and believed it to be the first, felt justified in questioning the existence of such an edition altogether. But the discovery of two copies here, and the subsequent examination of a copy by Dr. Kopp himself, as he informed me, put an end to the possibility of doubt on the matter.†

3. Notwithstanding the existence of an edition of 1572, and reference to it by certain authorities, it appears to have been so rare that it was hardly known. That of 1573 is still rarer, for it is not mentioned at all. At all events, both were unknown to Reinesius,‡ who mentions an edition of Cologne 1574 only, but in a very vague way. This last is, indeed, confirmed indirectly by Conring, who, speaking of the work, says that it first appeared at Rome in 1570, and was reprinted four years later at Cologne. But as his initial statement requires the strongest of all confirmations, namely, the existence of an actual copy dated Rome, 1570, his somewhat loose remark that the book appeared four years later might not be meant to be interpreted quite strictly. When, therefore, I said§ that the existence of a 1574 edition was quite possible, although I had seen no copy anywhere mentioned, it seemed to me so barely probable that an edition of Mizaldus' Secrets, with Democritus and Synesius appended, should appear at Cologne in three successive years, 1572, 1573, 1574, by the same printer, Birckmann, that I did not feel justified in removing the query from the 1574 edition in the list which I gave.‖

4. Here again I have been shown by facts the error of a too sweeping doubt, for I have within the last week ascertained that a copy of the work, dated 1574, is in the University Library at Cambridge, and by the kindness of the librarian, Mr. Jenkinson, I have been able to examine it, and compare it with the edition of 1572.

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* *Proceedings of the Philosophical Society of Glasgow*, 1885. Vol. xvi., p. 38.

† March, 1891.—I have since seen another copy of the 1572 edition of Mizaldus in the Library of Trinity College, Cambridge.

‡ *Ibid.*, p. 38. § *Ibid.*, p. 43. ‖ *Ibid.*, p. 44.

The following is a description of this volume similar to what I gave of the 1572 edition in the previous paper (§ 9).

5.

ANTONII MIZALDI MON-
luciani Galli, Medici,
MEMORABI-
LIVM, SIVE ARCA-
NORVM OMNIS GE-
NERIS,
PER APHORISMOS DI-
gestorum, Centuriæ IX.
ET
DEMOCRITVS ABDERITA, DE
rebus Naturalibus, & Mysticis.
Cum
SYNESII, ET PELAGII
Commentarijs.
Interprete de Græca lingua,
Dominico Pizimentio Vibonen-
si Italo.
Præfatio,
In omnes hosce libros.
COLONIAE,
Apud Ioannem Birckmannum
Anno D.M.LXXIIII.
Cum Gratia & Priuilegio Cæsar. Maiestat.

24°. *Signatures in 12.* * Title; * 2*r* to ***7*v* [or 30 leaves] Præfatio *to Thomas Redinger, dated:* Calendis Martijs. M.D.LXXII. Vbiorum Colonia. ***8*r* to ****9*r* [or 14 leaves] Index Rerum præcipuarum. ****10, 11, 12, *are blank in this copy. The title and preliminary matter, therefore, occupy 48 leaves [3 blank] not numbered.*

The text is numbered consecutively from f. 1 to f. 245, or sigs. A *to* X6 *in 12's. Text stops on* X5 *v.* X6 *is blank.*

Collation: f. 1: Title; *f.* * *2 recto:* De Mizaldi | Arcanis, Nec Non | Graecis In Demo- | critvm, Caeteris- | q̃ue Chemiæ scripto- | ribus, | Præfatio. | Ad Clarissimum No- | bilitate, doctrina, prudentiaqúe virum, | Thomam Redingervm | Silesivm. |

This preface ends on ****7v. or f. 31v. with the words:* compara- | tur. Vale. Calendis | Martijs. M.D. | LXXII. Vbio | rum Colo- | niæ | (·.·) | *with no scroll ornament.* On ***8*r or f.* 32*r begins* Index Rervm, *which ends on* *****9r or f. 45 recto. Verso is blank, and three blank leaves follow, completing the signature. This ends the introductory matter, which is not numbered. The text of Mizaldus' treatise then begins on leaf A, f. 1, and goes down to the recto of f. 212. Verso is blank.*

F. 213r contains the title to the Chemical Tracts as follows:—

Ex | Venerandæ Græcæ vetustatis de ar- | te Chymica, relliquijs. | Democritvs | Abderyta, De Arte | Sacra: Sive, De Rebvs | naturalibus & my- | sticis, | Necnon | Synesij, & Pelagij, Antiquorum | Philosophorum: in eundem, | Commentaria. | Interprete | Dominico Pizimentio Vibo- | nensi Italo. | *Small scroll ornament, same as in the 1572 edition. F. 213 verso is blank. F. 214 recto contains Pizimenti's preface, which ends f. 218 recto, with the words:* Stephani Alexandrini, Olympiodori, | & Pelagij cōmentaria, in eundem | Democritum propediem ex | pecta. Datum Romæ. | Calend. Septemb. | M.D.LXX. | (·.·) | *The catch word is* Ex. *Then F. 218 verso begins:* Ex Rebus Na- | tvralibvs Et My- | sticis Demo- | criti. | (·.·) | Natvra naturâ | gaudet: *&c., which ends f. 227 verso:* omisi, cùm liberè in alijs etiam | meis scriptis pertracta- | rim. In hoc scripto | valete. | (·.·) |

F. 228 recto: Synesii Phi | losophi Ad Dio- | scorvm In Librvm | Democriti. | Scholia, | (·.·) | *with a scroll ornament, but quite different from that in the edition of 1572. It ends f. 238 verso.*

F. 239 recto: Pelagii Philo- | sophi De Eadem | Divina Et Sacra | arte. | (·.·) | *Ends f. 245 verso, followed by a similar scroll ornament to that on f. 228, whereas the ornament in the 1572 edition is the same as that on f. 228 of the same edition. A blank leaf ends the volume.*

6. Comparison of this edition with that of 1572 shows, as was to be expected, that they are absolutely different throughout. The following are the main differences:—the 1572 edition contains signatures *, **, ***, ****, all in twelves, ***** in six; of this last sheet the fourth leaf contains Errata, and 5 and 6 are blank. A to X6 in twelves, X6 being blank.

The 1574 edition contains signatures *, **, ***, ****, all in twelves; this last sheet contains no Errata, and leaves 10, 11 and 12 are blank. A to X6 in twelves, X6 being blank.

In the 1572 edition, therefore, there are six leaves more than in that of 1574, but the distinction between them is much greater than that, for they differ typographically throughout.

7. But as regards the relationship of the 1573 and 1574 editions I am not as yet certain, until I am able to compare this 1574

copy with that of 1573 in the British Museum.* As far as the account of the latter which I have already given [*Proceedings, Phil. Soc., Glasgow,* 1885, vol. xvi., p. 40, § 9] shows, the 1574 edition is identical with it, except in the date. It is possible, therefore, that the 1574 is merely an issue of surplus copies of the previous year, with a re-dated title-page, but I would not say so positively without careful comparison. It is just as possible that the edition of 1574 is really new, a reprint, differing typographically from those of 1572 and 1573, as that the edition of 1573 is quite different from that of 1572.

8. Anyhow, the existence of an edition dated 1574 is demonstrated by an actual copy of it.† The statements, therefore, of Reinesius and Conring as to a 1574 edition are justified, and the query appended to that edition in the list I have given [*Proceedings, Phil. Soc., Glasgow,* vol. xvi., p. 44, § 14] may be deleted.

9. The discovery of this copy is of interest as showing how great the demand for these books must have been, when, in three successive years, 1572, 1573, 1574, there were three issues of them from the press of Birckmann at Cologne, two of which certainly are different from each other. It also gives a fresh illustration of the great rarity of all these books, when of them, one, that of 1573, was quite unknown until I described it, and I know now only one copy of it; that of 1572 was denied by Dr. Kopp, and I know of only three or four copies of it; while that of 1574 was questioned by me because the authorities for it appeared themselves to be vague as to its existence.

10. The preceding was all I had to put before the Society when I gave notice of these remarks as supplementary to my former paper; but since then, only yesterday, I received the very interesting information that in the Cambridge University Library

[* I was not able to make this comparison till February, 1891. I have given the results at the end of the present paper.]

† March, 1891.—I have since ascertained that there is a copy of the 1574 edition in the University Library, Aberdeen, which, by the kindness of the librarian, I have been able to examine.

there is a copy of the edition of Democritus and Synesius printed at Padua in 1573. This was the edition described by Beckmann,* from the copy in the Göttingen University Library. It was also the only edition of the work which Dr. Kopp† had before him in 1867, and which, after many inquiries, he, too, found at last in Göttingen. At the time he considered this the first edition, those of Cologne and Padua, dated 1572, being entirely doubted by him. It was described by Fabricius and other later historians and bibliographers, but it is doubtful if any of them ever saw a copy of the edition. So that, as I formerly showed,‡ its existence was authenticated by Birckmann at the end of last century, and by Dr. Kopp twenty years ago, both of them using the same copy. It was from this that Dr. Kopp made the reprint in his *Beiträge.*

The following is a description of the copy in the University Library, Cambridge:—

> Democritvs | Abderita | De Arte | Magna, | Siue de rebus naturalibus. | Nec non Synesii & Pelagii, & Stepha- | ni Alexandrini, et Michaelis Psel- | li in eundem commentaria. | Dominico Pizimentio Vibonensi | Interprete. | [*Device.*] | Patavii | Apud Simonem Galignanum | MDLXXIII. |

It is a small octavo volume, signatures a to i, or 70 numbered leaves and 2 blank leaves. The collation is as follows:—a1, title; a2 recto to a5 recto, or ff. 2 recto to 5 recto, Pizimenti's address to Cardinal Antonius Perenottus; a5 verso, the text begins, and ends i6 recto, or ff. 5 verso to 70 verso; i7 and 8 are blank leaves. The device on the title-page is an anchor enwreathed by a serpent, and grasped by two right hands issuing from clouds.

11. It is of excessive rarity. This is the only copy I know of in this country, and the Göttingen one seems to be the only known copy in Germany. There is no copy in the British Museum, Bodleian, or any other library that I have been able to examine. Every writer who has spoken of it has emphasised its rarity. I am, therefore, very fortunate in being able so unexpectedly to add to my present communication an account from an actual copy

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* *Beiträge zur Geschichte der Erfindungen*, Leipzig, 1792, III., p. 376; in the English translation, London, 1814, III., p. 66.

† *Beiträge zur Geschichte der Chemie*, Braunschweig, 1867, p. 113, note 22.

‡ *Proceedings of the Philosophical Society of Glasgow*, 1885, vol. xvi., p. 37, § 3.

of this so rare book, the principal one, too, connected with the subject.

12. The existence of two independent editions, so to speak, —that of Cologne, 1572, and of Padua, 1573—makes me more than ever inclined to believe in the possibility of an earlier edition printed in Italy than any of those yet described. Two there may be—Rome, 1570, mentioned by Conring, which is the earliest possible, and Padua, 1572, mentioned by Ducange, Fabricius, and Mullach. After Conring having proved to be right about a Cologne edition dated 1574, I am more inclined to think it possible that he may be again correct as to an edition printed at Rome in 1570, but no copy is as yet forthcoming. As to a 1572 Padua edition, denied by Beckmann and by Dr. Kopp, it is quite as likely (as I have already said*) that there were editions issued there in 1572, 1573, as that there were, for certain, Cologne issues of 1572, 1573, 1574. I am not without hope that I may at some time or other be able to communicate to the Society an account of one, if not both of these editions. It would not be less improbable than this present account of the 1574 edition, of which I was myself most doubtful six years ago, and of the most rare Padua edition, of which I had no expectation of ever coming across a copy.

13. The result at present is that of the nine editions of Democritus quoted from all known authorities in my former paper, descriptions of six of them from actual copies have now been laid before the Society. Three still remain doubtful—Rome, 1570; Padua, 1572; Nürnberg (in German), 1717.

POSTSCRIPT. *November* 28, 1890.

Since the preceding was read to the Society, I have been enabled, by the kindness of the librarian, to examine the Cambridge University Library copy of the Padua edition, and find it far more interesting than I could have supposed, so much so that I have had a photo-facsimile made of the title-page to render my remarks

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* *Proceedings of the Philosophical Society of Glasgow*, 1885, vol. xvi., p. 42, § 10.

intelligible (Plate III). Two things will be noticed: 1st, the misprint Ibderita, corrected to Abderita by printing a larger A over the I, but obliquely so as to obliterate it as much as possible; and 2nd, the position of the middle I in the date. It looks as if the date had been originally MDLXXII, and that another I had been stuck in afterwards. This seems to me to be conclusive. The work really appeared at Padua in 1572, and then it was re-dated and issued in 1573. It is impossible, of course, to say whether all the copies were re-dated, but the only two that I know of at present seem to have been dealt with in the same way. It *is* possible, however, that some escaped, so that there is a chance that such a copy may be met with, but the present copy, from the way the date has been altered, seems to me to leave no doubt that the book was issued originally in 1572.

This simplifies the entire subject, and I am gratified that the inspection of the book has confirmed my anticipations.

I must reserve for a subsequent paper a fuller account of this edition, and a comparison of it with that of Cologne, 1572.

APPENDIX. *February* 25, 1891.

In my original paper I described the British Museum copy of the Cologne 1573 edition of Mizaldus' *Memorabilia*, but with less detail than that of 1572. I now give a fuller account of the 1573 edition, for comparison with that of 1574. After minute examination of the 1573 and 1574 issues, I can detect no difference except in the date and in the dislocation of signature B2 (whereby the 2 has got separated from the B) in the 1574 edition.* So far as I can see, the 1574 edition is simply a reissue of surplus copies of the 1573 edition with altered date, for slips that would have been corrected in an entirely new edition are left unaltered: thus, signature S5 wants the 5 in both issues; folio 228, line 1, the I in PHI... is similarly defective in both, and in many similar minute points they agree.

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* This dislocation, I find, is only in the Cambridge University Library copy. The Aberdeen copy, which I have since examined, is quite regular.

DEMOCRITVS ABDERITA DE ARTE MAGNA,

~~N 11 24~~

Siue de rebus naturalibus.

Nec non Synesii, & Pelagii, & Stephani Alexandrini, & Michaelis Pselli in eundem commentaria.

Dominico Pizimentio Vibonensi Interprete.

PATAVII
Apud Simonem Galignanum
M D LXXIII.

Maclure, Macdonald & Co.

ANTONII MIZALDI MON-
luciani Galli, Medici,

MEMORABI-
LIVM, SIVE ARCA-
NORVM OMNIS GE-
NERIS,
PER APHORISMOS DI-
geſtorum, Centuriæ IX.

ET

DEMOCRITVS ABDERITA, DE
rebus Naturalibus, & Myſticis.

Cum

SYNESII, ET PELAGII
Commentarijs.

Interprete de Græca lingua,
Dominico Pizimentio Vibonen-
ſi Italo.

Præfatio,
In omnes hoſce libros.

COLONIAE.
Apud Ioannem Birckmannum
. Anno D.M.LXXIII.

Cum Gratia & Priuilegio Cæſar. Maieſtat.

24°. *Sigs. in 12.* *Title; *2*r* to ***7*v* [or 30 leaves], Præfatio *to Thomas Redinger, dated:* Calendis | Martijs. M.D. | LXXII. Vbio | rum Colo- | nia (·.·); ***8*r* to ****9*r* [or 14 leaves], Index Rervm | præcipuarum; **** 10, 11, 12, *are wanting in the Museum Copy. The title and preliminary matter, therefore, occupy 48 ff. [3 blanks wanting] not numbered. The text is foliated consecutively from* f [1] *to* f. 245, *or sigs.* A *to* X6 *in* 12's. *Text stops on* X 5 *verso*, X 6 *is blank. Sig.* B. *runs quite uniformly, and* B2 *is not dislocated.*

Collation: f. 1, *Title, v. blank.*

*2 *recto:* De Mizaldi | Arcanis, Nec Non | Graecis In Demo- | critvm, Caeteris- | ꝗue Chemiæ scripto- | ribus, | Præfatio. | Ad Clarissimvm No- | bilitate, doctrina, prudentiaqúe virum, | Thomam Redingervm | Silesivm. |

This preface ends on ***7 *v.* (= *f. 31 v.*) *with the words:* comparatur. Vale. Calendis | Martijs. M.D. | LXXII. Vbio | rum Colo- | nia (·.·). *With no scroll ornament. On f. 32 r. begins* Index Rerum, *which ends on f. 45 r. Verso is blank, and the three blank leaves to complete*

the signature are wanting. The introductory matter is not numbered. The text of Mizaldus' treatise then begins on leaf A1, f. [1], *and goes down to the recto of f. 212. Verso is blank.*

F. 213 r. contains the title to the Chemical Tracts, as follows:—

Ex | Venerandæ Græcæ vetustatis de ar- | te Chymica, relliquijs. | Democritvs | Abderyta, De Arte | Sacra: Sive, De Rebvs | naturalibus & my- | sticis, | Necnon | Synesij, & Pelagij, Antiquorum | Philosophorum: in eundem, | Commentaria. | Interprete | Dominico Pizimentio Vibo- | nensi Italo. | *Small scroll ornament, as in 1572.*

F. 213 verso is blank.

F. 214 recto contains Pizimenti's preface: Ad Amplissi- | mvm Illvstris- | simvmqve Seqva- | num, | *which ends f. 218 recto, with the words:* Stephani Alexandrini, Olympiodori, | &. Pelagij cōmentaria, in eundem | Democritum propediem ex | pecta. Datum Romæ. | Calend. Septemb. | M.D.LXX. | (·. ·) | *The catchword is* Ex. *Then*

F. 218 verso begins: Ex Rebvs Na- | tvralibus Et My- | sticis Demo- | criti. | (·. ·) | Natvra naturâ | gaudet: *&c., which ends f. 227 verso:* omisi cùm liberè in alijs etiam | meis scriptis pertracta- | rim. In hoc scripto | valete. | (·. ·) |

F. 228 recto: Synesii Phi | losophi Ad Dio- | scorvm. In Librvm | Democriti. | Scholia, | (·. ·) | *followed by the same scroll ornament as in 1574. This ends f. 238 verso.*

F. 239 recto: Pelagii Philo- | sophi De Eadem | Divina Et Sacra | arte. | (·. ·) *ends f. 245 verso, followed by a similar scroll ornament to that on f. 228.*

A blank leaf ends the volume.

XXI.—*The Meteorological Results of the "Challenger" Expedition in relation to Physical Geography.* By Alexander Buchan, M.A., LL.D., F.R.S.E., Secretary of the Scottish Meteorological Society.

[Read before the Society, 29th April, 1891.]

It can scarcely be said to be longer than twenty years since anyone was in a position to look at the various problems presented by the atmosphere considered as affecting the whole earth. Previous to that time much, no doubt, had been done and many results arrived at that will always be accepted as conclusive, but investigations before then were chiefly confined to investigation of isolated points or climatological investigations on restricted regions of the earth. Any problem affecting the earth, as a whole, could not, previous to that time, be satisfactorily discussed, because we were not in possession of the data of observation to do so. But a step in advance was taken about that time in attempting to deal with the atmosphere as a whole, in its relation to the globe. Nearly the whole bulk of the meteorological work which had been done referred only to the land. There was but little known of the ocean, so little that really we could not take the ocean into account in investigating the broad questions of climatological research. Many ideas of the wildest description prevailed, such as, that all sea temperatures at a great depth were uniform at 39°; the temperature of the surface of the ocean by night and by day, the winds that blew over it, and the currents resulting therefrom, we knew little of, and certainly nothing accurately.

So, then, it happened, when the *Challenger* expedition was contemplated, and arrangements were made to settle the biological and physical work to be done by that great expedition, that it was resolved that the temperature of the ocean should form one of the serious objects of the expedition. Arrangements were made to carry this out, with the result that for every two hours—and when in the higher latitudes of the southern ocean every hour—the most complete meteorological observations were made of the

air and the surface temperature of the ocean. But you are aware that, in addition, the *Challenger* expedition went greatly beyond this, such as in taking observations of deep-sea temperatures, which are amongst the most valuable contributions that have been made to science in recent years, showing, amongst other things, the presence of ice-cold water at the bottom of every deep sea, even in equatorial regions. The results raised very wide questions, such as that of oceanic circulation, it being plain that ice-cold water in the tropics must come from higher latitudes through a circulation constantly kept up. To investigate this question, it was resolved to submit the meteorological observations of the *Challenger* expedition, and observations hitherto made over the globe, to a complete examination, with the view of determining this point: "What is the distribution of the earth's atmosphere in different seasons over the whole globe, sea as well as land?" The importance of this investigation it is impossible to overrate, but it is seen at once that all wind is simply a flowing away of the atmosphere from where there is a high barometer towards where there is a lower barometer at that time. Meteorology does not show any exception to that. Hence, then, if we are able to map out by lines the distribution of the mass of the earth's atmosphere, we know approximately the winds, even where we have no observation. But along with these were collected valuable observations of wind direction, and these are a test of the correctness of what has been advanced. It was ascertained nearly twenty years ago that the currents of the ocean are simply the effects of surface currents. In these circumstances I was asked to undertake this investigation, and to add to it all observations that were available to bring the whole globe, sea and land, under one view—a work which I have been engaged on for the past seven years.

The main results are given in the "*Challenger* Report," which includes fifty-two large maps, showing by isothermals the temperature on hypsobathymetric maps, and by isobars for the months of the year showing the pressure of the atmosphere, and by arrows the prevailing winds over the whole globe. It was judged advisable to give on a larger scale the part of the northern hemisphere surrounding the North Pole in addition. Some time ago I read before this Society a paper on the diurnal results of this work. One of the most remarkable results arrived at was this—that in all oceans, even in the tropics, the diurnal

variation of the temperature of the surface water of the sea does not quite amount to 1° Fahrenheit where there is deep water. Of course, going down the Clyde, where the sea is comparatively shallow, for instance, you have warm water on a hot day. But in the deep sea the surface temperature does not vary more than 1° Fahrenheit in any part of the ocean—or, more correctly, only about eight-tenths of a degree. This amount diminishes as we near the poles. On getting into the higher latitudes in the South Sea the daily variation was only two-tenths of a degree. Now, this result alters the whole complexion of a large number of physical questions relating to the atmosphere. Take, for instance, the diurnal range of the barometer. It has hitherto been the practice to discuss these phenomena, using as a datum the temperature of the air over the earth's surface where the observations have been taken, but the *Challenger* shows that the average surface of the sea has the diurnal variations of the barometer as marked as on land. We must, therefore, look for the cause of this great diurnal phenomenon not on the surface of the earth, but through the whole depth of the atmosphere.

Another remarkable result is this, that the temperature of the air on board the *Challenger*, resting on the surface of the sea, was from three to four times greater than that of the surface of the sea itself—that is to say, the sun heats up the air resting on the ocean to a much greater degree than it heats up the surface of the ocean itself. Another point was brought out in reference to the diurnal range of the barometer. Where there exists permanent high pressure, as, for example, on the west of Africa, and immediately to the west of North America, we have a less diurnal variation than anywhere else in the same latitudes over the ocean. But on the east side of the continents there are no such areas of high pressure and small diurnal range of pressure. The whole facts show that where the sun shines on the atmosphere in anticyclonic regions, or where high pressures prevail, there occurs only one-third or one-fourth of the usual amount for the same latitudes where these high pressures do not exist.

All meteorologists now believe that the winds flow out from these spaces in every direction, and since the pressure never becomes less, the whole of those spaces are necessarily filled with a vast descending current of air. This descending air is subjected continually to more pressure. All observations show the fact that

thunderstorms occur when the temperature falls very much, more rapidly with height than the normal fall; and thus thunderstorms are preceded by warm, close weather,—unusually warm, unusually moist. Over land 80 per cent. of the thunderstorms occur between 12 noon and 5 in the afternoon, or at the time when the land is most highly heated, when the temperature of the surface of the land is hottest, and when the temperature of the air resting upon it is the maximum of the day. On the other hand, we find that the diurnal maximum of thunderstorms over the ocean is very early in the morning from about 2 o'clock till 6, equally as marked as from 12 till 5 on land. The surface temperature of the sea during the day serves perceptibly to bring about much greater heat on the lower strata of the atmosphere than elsewhere, but during the night the higher part of the atmosphere is influenced by the terrestrial radiation, resulting in a greater relative cooling of the higher part of the atmosphere during the night thus bringing about the abnormal distribution of temperature.

[Maps were exhibited showing the distribution, during the months of the year, of the pressure, temperature, and prevailing winds of the globe, which were more fully detailed for January and July, the two extreme months of the year.]

In January, when, in the northern hemisphere, the sun's heat is least felt and the effects of terrestrial radiation are at the maximum, the highest pressure and the lowest temperature are to be met with in the interior of the great Eurasian Continent, from which the prevailing winds flow outward to the ocean all round. The lowest mean temperature anywhere or at any season observed is $-61^\circ{\cdot}2$ at Werkojansk (lat. 67°·34 N., long. 133°·51 E.), in North-Eastern Siberia; and it is at this place where a temperature of $-88^\circ{\cdot}8$ occurred in January, 1886, being absolutely the lowest temperature yet known to have occurred on the globe. The lowest mean temperature in America is only $-40^\circ{\cdot}0$ over a region to the north of the magnetic pole. No such low, mean, or extreme temperatures are known to occur anywhere in the southern hemisphere at any season.

On the other hand, in July the influence of the sun is most strongly felt, and the effects of terrestrial radiation are at the minimum. At this time, pressure is least and temperature highest in the Eurasian Continent; but these results are most pronounced, not in Siberia, but in the North-West Provinces of India. With these changed conditions, the prevailing winds are from the ocean

all round toward the interior of the Continent, carrying with them the copious or generous rains which are the great fertilisers of these countries. The highest mean temperature is a little over 95°·0 over the central part of Africa, over large parts of Arabia, Persia, and eastward to the Indies.

The same relations appear in the climatologies of South America, South Africa, and Australia, with the recurrence of season, but marked with characteristics much less pronounced owing to the smaller proportions of these continental masses. [The remarkable influence of the Red Sea on the distribution of pressure, temperature, and prevailing winds was pointed out by the author.] This is most strikingly shown in July, when temperature is nearly 20°·0 lower over this sea than it is in Africa to the west and in Arabia to the east of it.

It is scarcely possible to overstate the enormous influence of the ocean and its connected winds on the temperature everywhere, and at all seasons. Amongst the more striking illustrations are the winter contrast offered by the coast of Labrador, to the west coasts of Europe, in the same latitudes; and the influence of the warm winds from the Atlantic in maintaining at a higher figure the winter temperature of Siberia, as far eastwards as longitude 80° E. Indeed, until that longitude is reached, the isothermals follow, roughly speaking, a north-and-south, instead of an east-and-west course.

One of the most important results of this discussion is a tolerably complete representation of the winds of the terraqueous globe in all months, and the inevitable relations of these to the rainfall. It is now made plain, that the regions characterised by the heaviest rainfalls are just those parts of the globe whose prevailing winds have traversed the widest extent of ocean, and pass into higher latitudes or colder climates. Hence, are readily explained, the summer rainfalls of India, the Eastern Peninsula, China, Japan, and the United States; and the winter rains of North-Western Europe, and the North-Western shores of North America; and where the continental masses of land are of comparatively small extent, as in Australia and South America, and the isobaric systems are therefore less likely to be well developed year by year. It is in such regions where disastrous droughts from time to time occur.

The desert of Gobi is caused by quite a different set of meteorological conditions. During winter, the wind system of this region

is part of the general atmospheric movement of Asia, which proceeds from the centre of the continent in all directions toward the ocean. Hence, the region in this season is practically rainless; and during summer the winds become northerly as they follow the general atmospheric movement in upon the low pressure system in North-West India; and, hence, in this season also, the climate is rainless. The point emphasised here is that these regions are made deserts by the meteorological conditions of pressure, with their resulting prevailing winds, which, in their turn, are the inevitable result of the present distribution of land and water over the face of the earth.

What may be called the permanent anticyclonic regions of the globe, situated in the ocean to the westward of the great continental masses about latitudes 20° to 40° N. and S. hemispheres, play a first part in the role of the distribution of the rainfall. An examination of the surface winds of the anticyclones in their middle and southern portions in the northern hemisphere, and in their middle and northern portions in the southern hemisphere, shows clearly that they traverse but a short way over the ocean before reaching the continent adjoining; and, further, as they thereafter pass into lower latitudes, they necessarily become warmer as they advance. The inevitable consequence is that the parts of North and South America and the North and South of Africa, where these anticyclonic regions prevail, are virtually rainless, barren deserts; and it may be stated, with absolute confidence, that they will remain deserts as long as the present geographical distribution of land and water remains substantially as it is. On the other hand, as the winds which blow out of those anticyclones and thence in upon the continent on the side farthest from the equator must arrive at the land, after having traversed a considerable extent of ocean, and thereafter advance into higher latitudes, and, therefore, colder climates, the rainfall over such regions is large in amount, and, in many cases, even in undue excess. The dry arid climate of Lower California, as contrasted with the West Coast of British Columbia, with the respective rainfalls of 10 inches at San Diego and 100 inches at Fort Simpson, well illustrate the point.

XXII.—*Memoir of the late* MR. ALEXANDER WHITELAW. By the SECRETARY.

[Read before the Society, 15th April, 1891.]

SPEAKING not only on my own behalf, but also on behalf of the Council and of many other members of the Society who had the pleasure of his acquaintance, I have to state that it was with very great regret we learned a few weeks ago of the sudden death of Mr. Alexander Whitelaw, who was for so many years one of us—a zealous and faithful member of this Society, into which he was elected so far back as 10th January, 1855, during the Presidentship of the late Dr. Allen Thomson, F.R.S.

Alexander Whitelaw was the elder son of Mr. George Whitelaw, and was born in Glasgow—South Side—on 5th October, 1829. His father was an engineer of marked originality and very considerable scientific attainments. For a long time he was connected with a famous South Side engineering firm—namely, that of Messrs. D. Cook and Co., as manager of their works, and latterly he was a partner of the firm. A well-known member of our Society, who was draughtsman under him, and had an excellent opportunity of forming a correct estimate of him, tells me that he was about the most ingenious mechanical engineer with whom he had ever come in contact. One of his inventions was a new method for regulating the supply in marine and high-pressure boilers, for which he received the silver medal of the London Society of Arts. The other was a new apparatus for raising water, and for his invention he received the large silver medal of the same Society. Both of these awards were voted to him in the session 1832-33—the short papers in which he described them being written, respectively, in 1830 and 1832. Mr. George Whitelaw also played a prominent part in devising the steam rivetting machine with which the name of D. Cook and Co. was intimately associated. But it is of Alexander Whitelaw, rather than his father, that I have to speak, and to him I must now return.

After completing his ordinary school education, part of which he got in company with our respected Treasurer, Mr. Mann, he betook himself to the study of chemistry—first in Anderson's College, under the late Dr. Penny, and subsequently under Professor William Gregory, in the University of Edinburgh. When he was about twenty years of age, young Whitelaw obtained a situation in the soap works of Messrs. James Boyd & Co., in whose service he remained for about two years.

In the year 1852 Mr. Whitelaw started business as a soap manufacturer on his own account in Sydney Street, off Gallowgate. In the following year the duty on soap was repealed, and that circumstance gave great scope for the application of new methods, the result being a great impetus to this branch of the chemical industries so largely practised in the Glasgow district. Mr. Whitelaw took a very deep interest in the science as well as the art of his manufacture, and he very soon became a leading authority in his favourite department of technology. In or about the year 1869, owing to the fact of his works being situated in the line which the proposed City Union Railway aimed at taking with its Bellgrove connection, Mr. Whitelaw was compelled to quit his premises; and further north in the same street he erected new works, which he fitted with all the most approved appliances of the day.

Our late fellow-member took a keen and lively interest in the Philosophical Society, which he joined, as already mentioned, in the year 1855. He was not much given to the work of preparing papers to be read at the meetings of our Society, but he did on one occasion depart from the practice of being almost a "silent" member. That was in the Session of 1863-64, not long after Professor Graham (a former Vice-President of this Society) had been instrumental in creating much interest amongst chemists and physicists regarding the principle of Dialysis in chemical research. Mr. Whitelaw took up that subject from the point of view of a practical chemist, and he made a communication to the Society at that time on "A Practical Application of Dialysis," which afterwards appeared in the *Chemical News* for 26th March, 1864. The application consisted in separating the excess of common salt from the brine which results from salting meat so as to obtain a fresh and wholesome meat extract. This was done by suspending the brine enclosed in bags made of the skins of animals in fresh water until, by dialysis, the desired freedom from salt

was obtained. By this process Mr. Whitelaw proposed to utilise profitably the large quantities of brine which were wasted in curing establishments.

When the formation of a Chemical Section within the Society was agitated amongst the members who were more or less intimately connected with chemical pursuits, he took an active part in the movement, and he was one of the members of the first Council elected to guide and watch over the interests of the Section. Almost ever afterwards he was in the Council, and for two separate terms he served as Vice-President of the Section. Mr. Whitelaw also served two separate terms of three years in the Council of the Society—1872-75 and 1884-87. Though never a demonstrative member, he diligently and faithfully discharged the duties of the office, and gained the confidence and esteem of his colleagues.

Mr. Whitelaw was also a zealous supporter of Anderson's College, in the full remembrance that within its walls he received much valuable scientific instruction in his student days. For a number of years he took a very active part, as one of the managers, in the administration of the affairs of the "Andersonian"; and when that most valuable institution ceased to have a separate existence and was merged in the Glasgow and West of Scotland Technical College, he became one of the Life Governors of the new institution, in which he took and performed his full share of the work devolving upon the administrators of the Technical College Scheme, like his and our other late much-esteemed colleagues, Mr. J. J. Coleman and Dr. Henry Muirhead.

Our lately-deceased friend always took a very keen interest in the progress of science, more especially in the departments of chemistry and physics; and he kept himself well abreast of that progress by reading copiously in the scientific journals, and by careful study of many leading scientific works. He manifested his interest in science and technology by becoming early, and remaining intimately, connected with the Society of Chemical Industry, the Glasgow section of which he assisted to manage as a member of the committee. He likewise mixed much with scientific people from all parts of the kingdom at the annual meetings of the British Association.

Throughout most of his life Mr. Whitelaw enjoyed excellent health, but just about a year ago he became dangerously ill through an attack of apoplexy, from which, however, he made a wonderful

recovery; but a recurrence of the same ailment overtook him just as he was entering the New Club, Glasgow, on 13th March of this year, with the result that, on the eve of the same day, after an illness of seven hours, he passed over to the majority in the sixty-second year of his age. To mourn his too-early death he has left behind him a widow and a grown-up family of two sons and three daughters.

JOHN MAYER.

ABSTRACTS.

Professor James Blyth, M.A., F.R.S.E., Glasgow and West of Scotland Technical College, made two short communications to the Society during the session. The first, on 8th January, 1891, was on "A Combined Windmill and Dynamo." It consisted, he said, essentially of the common form of windmill, with this difference, that a large flywheel with three V grooves was attached to the end of the wind shaft. From the platform separating the wind shaft depended a strong bracket, to which the dynamo was attached, so as to be able to rotate just clear of the ground along with the windmill as the wind changed. The dynamo was driven by ropes directly from the large flywheel. The author also described a form of centrifugal governor, by means of which the storage cells were thrown out of circuit when the windmill stopped or ran at too slow a speed for storage. He also described a contrivance by means of which one, two, three, or any number of wheels could be charged in series according as the velocity of the wind increased or decreased.

On 2nd April, 1891, he exhibited an experiment in self-induction. It consisted in sending a periodic current got by a tuning-fork interruptor through two conducting circuits arranged in parallel. The one circuit contained a monochord wire, and a coil whose self-induction could be altered; while the other contained a horse-shoe electro-magnet, placed astride the wire so that the latter was free to vibrate in the narrow gap between its poles. It was shown that the wire sounded loudly in unison with the fork when the phases of the two currents agreed, but that it stopped entirely when the phases were made to differ by altering the self-induction of the circuit. It was pointed out that, by an arrangement such as this, co-efficients of induction could be compared.

REPORTS OF SECTIONS.

SESSION 1890-91.

[Received at Meeting of Society, 29th April, 1891.]

1. Report of Architectural Section.

During the Session eight Meetings have been held, at which the following papers were read :—

Monday, 17th November, 1890.—Opening Meeting, when Mr. James Thomson, architect, F.R.I.B.A., President of the Section, gave his Address.

Monday, 1st December, 1890.—Mr. Campbell Douglas, architect, F.R.I.B.A., read a paper on "Some Architectural and other Notes on the Iberian Peninsula," illustrated by Lime-Light Views.

Monday, 15th December, 1890.—Mr. A. Lindsay Miller, architect, read a paper on "A Sketch of the History of Architecture," illustrated by Lime-Light Views.

Monday, 19th January, 1891.—Mr. Thomas Main read a paper on "Fireplace Construction."

Monday, 2nd February, 1891.—Mr. Francis H. Newbery, Head Master Glasgow School of Art, read a paper on "The Artist in the Architect."

Monday, 16th February, 1891.—Mr. James Paton, F.L.S., read a paper, subject—"An Art Gallery: its Architectural Requirements." (Page 128 of *Proceedings.*)

Monday, 2nd March, 1891.—Mr. John Gordon, architect, read a paper on "The Principles of Utility in Architecture."

Monday, 16th March, 1891.—Mr. Robert Hill, W.S., read a paper on "The Incidence of Local Taxation."

The thanks of the Section are due to those gentlemen.

During the Session five Associates joined the Section.

The Annual Business Meeting was held on Monday, 16th March, 1891, when Mr. James Thomson, architect, F.R.I.B.A., was re-elected President of the Section, and the Council was re-constituted. (See p. 342.)

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2. REPORT OF THE GEOGRAPHICAL AND ETHNOLOGICAL SECTION.

No papers from the Section were read before the Society during the Session; but five Meetings were held under the arrangement for holding joint Meetings with the Glasgow Branch of the Royal Scottish Geographical Society, at which the following papers were read:—(1) "Our Commercial Relations with China," by Prof. Robt. K. Douglas, on 19th December, 1890; (2) "From the Mouth of the Tana to the Source of the Nile," by Dr. Carl Peters, on 6th February, 1891; (3) "South Africa: Past and Present," by Rev. Dr. Stewart, of Lovedale, on 11th March, 1891; (4) "How Maps are Made," by Mr. W. B. Blaikie, Edinburgh, on 23rd April, 1891; and (5) "British Civilisation and Influence in Asia," by Prof. A. Vambéry, on 22nd May, 1891.

GEO. A. TURNER, M.D.,
Secretary.

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3. BIOLOGICAL SECTION.

4. CHEMICAL SECTION.

Both of these Sections are for the present suspended by Vote of Council, 26th November, 1890.

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5. REPORT OF THE MATHEMATICAL AND PHYSICAL SECTION.

The Section held no Meeting of its own during the Session; but several papers obtained through it, were read at the ordinary Meetings of the Society, and will be printed in the *Proceedings*.

MAGNUS MACLEAN,
Secretary.

6. Report of the Sanitary and Social Economy Section.

A Meeting of the Section was held on 5th November, 1890, at which the Office-Bearers for the year were elected.

Our President, Mr. W. P. Buchan, read two papers before the Society during the Session, the first on 19th November, 1890, "A Problem in Ventilation by Heat;" the second, as his Presidential Address, on 18th February, 1891, "On the progress of Sanitation, with special reference to the Sanitary Condition of the Glasgow Public Schools." It was hoped that the important subject of the Hygiene of Schools would have been followed up this Session with papers from Professor Hay, of Aberdeen, and Mr. John Fogie, F.C.S., of Dundee University College, on the medical and chemical aspects of the question, but these gentlemen, unfortunately, were unable to make arrangements for this Session. It is hoped that the subject may not be allowed to drop, and that it will be followed up next Session.

W. R. M. Church, C.A.,
Hon. Secy.

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7. Report of the Economic Science Section.

The Session has been marked by the reading of a number of valuable papers, as follow :—

"The Theory of Trades Unions," by T. S. Cree; Wednesday, November 12, 1890.

"Carlyle and Political Economy" (before the Society), by Dr. James Bonar, M.A.; Wednesday, December 3, 1890.

"The Principles of Taxation, with suggestions for Reform," by Mr. James M. Cherrie; Wednesday, January 14, 1891.

"Iceland: some Economic and other Notes" (before the Society), by Professor James Mavor; Wednesday, February 4, 1891.

"The Making of the Gold Reserves," by Dr. Charles Gairdner, President of the Section; Monday, April 27, 1891.

In addition to the above, the Section met on Monday, 4th February, to discuss the question of "The Scottish Railway Strike of 1890-91." The discussion was opened by Professor James Mavor, who made a statement pointing out certain lines which, in his opinion, should be followed in the discussion. A

number of members and associates took part in it. Two papers set down in the Syllabus for the Session were not read owing to the illness of the authors, namely :—"The Economic Functions of the £1 Note," by Mr. Alex. Cross; and "International Copyright," by Mr. J. D. Walker.

(Signed) WALTER W. BLACKIE,
Hon. Secy. of Section.

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8. REPORT OF THE PHILOLOGICAL SECTION.

There have been no Sectional Meetings during the past Session; one paper was contributed to the Society, namely :—

"The Poem of Béowulf, in the light of Recent Studies," by Dr. Charles Annandale.

At the Annual Business Meeting, held at 207 Bath Street, on Monday, 17th November, it was resolved to allow Prof. Jebb's name to stand as President for the Session. No satisfactory arrangement has yet been come to for the ensuing Session.

The sectional papers promised at the beginning of the Session have not been forthcoming, and no Associates' Subscriptions have been as yet received. The Section has incurred no expenses.

JAMES COLVILLE, M.A., D.Sc.,
Hon. Sec.

MINUTES OF SESSION.

5th November, 1890.

The Philosophical Society of Glasgow held its First Meeting for Session 1890-91, on the Evening of Wednesday, 5th November, 1890, at Eight o'Clock, in the Society's Rooms, 207 Bath Street —Dr. J. G. M'Kendrick, F.R.S., President, in the Chair.

1. The Minutes of Meeting held on 30th April, 1890, which had been printed in Vol. XXI. of the Society's *Proceedings*, now in the hands of the Members, were held as read, were approved of, and signed by the Chairman.

2. The President delivered the Opening Address, his subject being "On some of the Problems of Modern Physiology." At the close he was awarded the thanks of the Society, on the motion of Dr. J. B. Russell.

3. Mr. Henry Dyer, M.A., D.Sc., C.E., read a "Memoir of the late Dr. Henry Muirhead, Past-President," for which he received the best thanks of the Society.

4. Mr. Thomas S. Cree and Mr. John Farquhar were appointed to audit the Treasurer's Accounts for the year 1889-90.

5. The President announced that the following Candidates for admission into the Society had been duly elected:—Mr. C. O. Lundholm, manager, Nobel's Explosives Factory, Ardeer, Stevenston; Mr. John Bruce Murray, manager, State Line Steamship Company, 65 Great Clyde Street; Mr. Andrew W. Meikle, M.A., Viewfield House, Pollokshields; Mr. Thomas Shields, M.A., 12 Queen's Crescent, Cathcart; Mr. Thomas A. B. Carver, B.Sc., C.E., Oswald Hill, Partick; Mr. William Wallace, M.A., Allan Glen's School; Mr. Malcolm Sutherland, Leven Shipyard, Dumbarton; Mr. William Jolly, F.G.S., F.R.S.E., H.M. Inspector of Schools, 25 St. Andrew's Drive, Pollokshields; Mr. Andrew Bain, iron and steel manufacturer, 17 Athole Gardens; Dr. Moses Thomas, superintendent, Royal Infirmary.

19th November, 1890.

The Annual General Meeting of the Philosophical Society of Glasgow was held in the Society's Rooms, 207 Bath Street, on the Evening of Wednesday, 19th November, 1890, at Eight o'Clock—Dr. J. G. M'Kendrick, F.R.S., President, in the Chair.

1. The Minutes of the First Ordinary General Meeting for Session, 1890-91, which were printed in the Billet calling the Meeting, were held as read, were approved of, and signed by the Chairman.

2. The following gentlemen elected on 5th November were admitted to the Membership of the Society:—Mr. C. O. Lundholm, Mr. John Bruce Murray, Mr. Andrew W. Meikle, M.A., Mr. Thomas Shields, M.A., Mr. Thomas A. B. Carver, B.Sc., C.E., Mr. William Wallace, M.A., Mr. Malcolm Sutherland, Mr. William Jolly, F.G.S., F.R.S.E., H.M. Inspector of Schools, Mr. Andrew Bain, and Dr. Moses Thomas.

3. The Annual Report by the Council on the State of the Society, having been printed in the Billet convening the Meeting, was held as read. Its adoption was moved from the Chair, and unanimously agreed to. The Report is subjoined:—

Report of Council for Session 1889-90.

I. *Meetings.*—During the Eighty-seventh Session of the Society, which commenced on 6th November, 1889, and closed on 30th April, 1890, there were held thirteen meetings. One of these was held on 19th March in the Banqueting Hall, City Chambers, the use of which was kindly granted to the Society by the Lord Provost, Magistrates, and Town Council, for a lecture on "Public Lighting by Electricity," by Mr. Henry A. Mavor, Member of the Institution of Electrical Engineers. The hall was electrically-lighted, and the lecture was fully illustrated by experiments, by apparatus, and by lantern views. This meeting was largely attended by members of the Society, members of the Town Council, and friends, who were present by invitation. Another ordinary meeting of the Society was held in the Natural Philosophy Class-Room of the University, on 16th April, when, by request, Professor Oliver J. Lodge, F.R.S., of University College, Liverpool, delivered a lecture on "Electrical Oscillation," which was extensively illustrated by experiments of unusual interest. All the other meetings were held in the Society's Rooms. There were twenty-six communications, lectures, papers, &c., given to the Society during the Session. Of these, eighteen have been published, either in full or in abstract, in Vol. XXI. of the Society's *Proceedings*. In addition to the Society's own meetings, there were two joint meetings with the Glasgow Branch

of the Royal Scottish Geographical Society, at which papers were read by General Sir Lewis Pelly, K.C.B., and Mr. Paul du Chaillu.

II. *Membership.*—The number of Ordinary Members on the Roll at the beginning of the Session was 623. During the Session 53 new Members were admitted, and 5 have been re-instated from "Suspense List," making 681. Of these, 18 have resigned, 8 have died, 5 have left Glasgow and their names have been placed on the "Suspense List," and 3 have been struck off the Roll for non-payment of subscriptions—leaving on the Roll at the beginning of the present Session 647 Members, being an increase of 24. Of the 53 new Members, 8 became Life Members. There are now 109 Life Members. Two vacancies exist in the list of Honorary Members—the limit being twenty. There are 18 Honorary Members, of whom 6 are Continental, 3 are American or Colonial, and 9 are British. The number of Corresponding Members is 10. The Membership of the Society, then, is as follows:—Honorary Members, 18; Corresponding Members, 10; Ordinary Members, 647; or a total of 675.

III. *Sections.*—(1) Eight meetings of the *Architectural Section* were held during the Session. At these there were read seven papers, in addition to the Opening Address of the President. Full summaries of two of the papers are published in Vol. XXI. of the *Proceedings*.

(2) As regards the *Chemical Section*, the position of affairs remains unchanged. It is to be hoped that in the ensuing Session some effort will be made to provide papers of such a character as to justify the awarding of the Graham Medal.

(3) The *Sanitary and Social Economy Section* did little or no work during Session 1889-90, but there is a prospect of more activity being shown by the Section in the course of the present winter.

(4) Through the *Geographical Section* three papers, all suitable for publication in the *Proceedings*, were obtained, one of them being Dr. Thomas Muir's Address as President of the Section. This Section was specially active during the Session.

(5) Owing chiefly to the illness of the Secretary, the *Biological Section* did little formal work, but three communications, dealing with biological subjects, were made to the Society. These are printed in the new volume of the *Proceedings*.

(6) The *Mathematical and Physical Section* was the means of providing eight papers for the general meetings of the Society. Six of these are published in the *Proceedings*, mostly *in extenso*.

(7) Three papers from the *Economic Science Section* were read before meetings of the Society.

(8) The *Philological Section* provided two papers for the Society meetings. One of them, by Dr. Fiedler, on "Glimpses into Teutonic Antiquity" is published in full in the *Proceedings*. Since the issue of the Council Report for session 1888-89, Professor Jebb, the first President of the Section, has left Glasgow for Cambridge.

IV.—The Members now have in their hands Vol. XXI. of the Society's *Proceedings* in which are included twenty-six separate communications—addresses, full papers, and abstracts. The Council feel certain that the volume will maintain the reputation of the Society. In the illustration of the contents of the volume there are two Maps and six Plates, one of which is a Cyanotype Reproduction of a Seaweed, and is used as frontispiece to the volume. In addition there are a number of smaller illustrations in the text.

V. *Graham Lecture.*—The Triennial "Graham" Lecture, which was instituted chiefly through the efforts of the late Mr. J. J. Coleman, in connection with the Chemical Section of the Society, was delivered in March, 1890, by Professor A. Crum Brown, of the University of Edinburgh. It will doubtless be of much value to students of chemical philosophy in future years, inasmuch as it deals with some deeply interesting scientific problems which engaged the attention of Professor Graham (a former member of this Society) and other leading chemists of the present century.

VI. *Lecture by Mr. Muybridge.*—In connection with the Society, acting as the Glasgow Science Lectures Association Trust, there was delivered last winter, in the Queen's Rooms, a lecture on "The Science of Animal Locomotion," by Mr. Eadweard Muybridge, of the University of Pennsylvania. It was largely attended by members of the Society, and by the general public. Much help was given by the Art Club, the School of Art, and the Photographic Association to secure the success of the meeting. The expenses, which were very heavy, were partly defrayed by a vote from the Science Lectures Association Fund, in accordance with the terms of the Trust.

VII. *Finance.*—The Treasurer's Statement opens with a balance of £65 14s. 1½d., and closes with a Balance of £190 1s. 8d., showing an improvement in the Funds, during the year, of £124 7s. 6½d. It is believed that all current indebtedness of the Society up to 31st October, 1890, has been paid.

By order and on behalf of the Council.

(Signed) JOHN MAYER,
Secretary.

4. The Treasurer's audited Statement of the Funds of the Society, which had also been printed in the Billet, was next submitted by the Chairman, and its adoption was unanimously approved of. The Abstract of the Treasurer's Account of the Graham Medal and Lecture Fund, and that of the Science Lectures Association Fund, were also submitted and approved of. These Financial Statements are given on pp. 324-327.

Dr. ABSTRACT OF HONORARY TREASURER'S ACCOUNT—

			1889-90.	*1888-89.*
To BALANCE in Bank and Treasurer's hands from last year,			£65 14 1½	*£20 4 5*
,, SUBSCRIPTIONS to 31st October, 1890—				
53 Entry-moneys at 21s., ...		£55 13 0		*22 1 0*
Annual Dues at 21s.—				
Arrears, ...	£1 1 0			
For 1889-90, 479 Ordinary Members, ...	502 19 0			
,, ,, 45 New Members,	47 5 0			
		551 5 0		*563 17 0*
Life Subscriptions at £10 10s.—				
2 Old Members, ...	£21 0 0			
8 New ,, ...	84 0 0			
		105 0 0		*73 10 0*
			711 18 0	
,, GENERAL RECEIPTS—				
Corporation of Glasgow, Interest on "Exhibition Fund," £451 17s. at 4½ °/ₒ, for half-year to Martinmas, 1888 (now discontinued), ...		£0 0 0		*9 18 3*
Bank Interest, ...		7 17 6		*3 8 0*
Proceedings, Catalogues, &c., sold, . .		0 11 6		*0 15 3*
			8 9 0	
,, LECTURE ON "ANIMAL LOCOMOTION" by Mr. Eadweard Muybridge— Tickets sold, ...			47 16 6	*0 0 0*
,, SCIENCE LECTURE FUND—				
Grant for balance of expenses of Mr. Muybridge's Lecture, ...		£6 7 9		
Grant towards expenses of Professor Lodge's Lecture, ...		4 0 6		
			10 8 3	*0 0 0*
,, ARCHITECTURAL SECTION— 99 Associates' fees for 1889-90, at 5s., ...			24 15 0	*17 0 0*
,, CHEMICAL SECTION— Associates' fees, ...			0 0 0	*0 0 0*
,, ECONOMIC SCIENCE SECTION—				
2 Associates' fees for 1888-89, at 5s., .		£0 10 0		
34 Do. for 1889-90, at 5s., .		8 10 0		
			9 0 0	*9 15 0*
,, GEOGRAPHICAL AND ETHNOLOGICAL SECTION— Associates' fees, ...			0 0 0	*7 10 0*
,, MATHEMATICAL AND PHYSICAL SECTION— 1 Associate's fee for 1889-90, at 5s., ...			0 5 0	*1 15 0*
,, PHILOLOGICAL SECTION— 4 Associates' fees for 1889-90, at 5s., ...			1 0 0	*2 0 0*
			£879 5 10½	*£731 13 11*

Memo. by Treasurer.—The Amount invested by the Society in the Bath Street Joint Buildings up to 31st October, 1890, is £3,547 8 1½
whereof, Paid from Society's Funds, ... £2,047 8 1½
Do. Society's half of £3,000 Bond, . 1,500 0 0
£3,547 8 1½

J. M.

SESSION 1889-90, AND COMPARISON WITH SESSION 1888-89. **Cr.**

		1889-90.	*1888-89.*
By General Expenditure to 31st October, 1890—			
Salary to Secretary, ...	£75 0 0		*£75 0 0*
Allowance for Treasurer's Clerks, . .	15 0 0		*15 0 0*
		£90 0 0	
New Books & Periodicals, British & Foreign,	£106 16 9		*100 5 5*
Bookbinding, ...	41 10 11		*55 11 2*
Printing Circulars, *Proceedings*, &c., . .	141 0 0		*171 10 3*
Lithographs, Woodcuts, &c., for *Proceedings*, &c.,	23 19 6		*14 7 6*
Postage and delivery of Circulars, Letters, &c.,	35 6 5½		*37 16 5½*
Stationery, Diplomas, &c., ...	12 7 5		*7 19 2*
		361 1 0½	
Fire Insurance on Library for £5,400, .	£6 1 3		*6 1 3*
Postages, &c., per Secretary, £3; per Treasurer, £2 17s. 2d., ...	5 17 2		*5 17 9*
		11 18 5	
Joint Expenses of Rooms—Society's half of £346 11s. 11d., being Interest on Bond, Insurance, Taxes, Cleaning, Repairs, Lighting, and Heating; Salaries of Curator and Assistant, less half of £68 19s. 6d., Revenue from Letting, ...		138 16 2½	*150 10 3½*
,, Lecture Expenses—			
Mr. Muybridge on "Animal Locomotion,"	£54 4 3		*0 0 0*
Professor Lodge's travelling expenses, .	4 5 0		*0 0 0*
Scottish Geographical Society, Rent for two Joint Lectures with, ...	2 0 0		*3 0 0*
Reporting and Sundries, ...	2 17 6		*0 9 6*
		63 6 9	
,, Subscriptions to Societies—			
Ray Society, 1890, ...	£1 1 0		
Palæontographical Society, 1890, . .	1 1 0		
		2 2 0	*2 2 0*
,, Architectural Section—			
Expenses per Treasurer of Section, ...		10 18 8½	*11 3 9½*
,, Economic Science Section—			
Expenses per Treasurer of Section, . .	£8 1 1		
Printing Account, ...	0 12 0		
		8 13 1	*2 3 7*
,, Geographical and Ethnological Section—			
Expenses per Treasurer of Section, . .	£0 0 0		
Printing Account, ...	2 0 0		
		2 0 0	*3 9 0*
,, Mathematical and Physical Section—			
Expenses per Treasurer of Section, ...		0 0 6	*0 1 6*
,, Philological Section—			
Expenses per Treasurer of Section, ...		0 1 6	*3 0 8*
,, Sanitary and Social Economy Section—			
Expenses per Treasurer of Section, ...		0 6 0	*0 10 6*
,, Balances, viz.:—			
In Clydesdale Bank—			
Lodged, ...	£690 0 0		
Drawn, ...	500 0 0		
	£190 0 0		
In Treasurer's hands, ...	0 1 8		
		190 1 8	*65 14 1½*
		£879 5 10½	*£731 13 11*

Glasgow, *12th November, 1890.*—We, the Auditors appointed by the Society to examine the Treasurer's Accounts for the year 1889-90, have examined the same, of which the above is an Abstract, and have found them correct, the Balances being—in Clydesdale Bank One Hundred and Ninety Pounds, and in Treasurer's hands One Shilling and Eightpence.

(Signed) THOMAS S. CREE.
JOHN FARQUHAR.

JNO. MANN, C.A., *Honorary Treasurer.*

GRAHAM MEDAL AND LECTURE FUND.

Dr. ABSTRACT OF TREASURER'S ACCOUNT—SESSION 1889-90. Cr.

Dr.

CAPITAL AT 1ST NOVEMBER, 1889—		
Glasgow and South-Western Railway Co. 4 % Preference Stock in name of the Philosophical Society, in Trust,	£250 0 0	
Value of Die at H.M. Mint, - -	18 18 0	
		£268 18 0
Cash in Bank, - - - - - - -	£28 19 5	
Cash on hand, - - - - - - -	0 10 8	
		29 10 1
REVENUE—		
Dividend, April, 1890, less Tax, - -	£4 17 6	
,, Oct. ,, ,, - -	4 17 6	
Interest from Bank, - - - - -	0 9 9	
		10 4 9
		£308 12 10

Cr.

FOURTH TRIENNIAL GRAHAM LECTURE, on 5th March, 1890, by Professor A. Crum Brown, F.R.S., on "The Basicity of Acids."		
Fee and expenses, - - - - - - - - -		£20 17 0
CAPITAL AT 31ST OCT., 1890—		
Investment, *per contra*, - - -	£250 0 0	
Die, - - - - - - - - -	18 18 0	
		£268 18 0
BALANCE, BEING REVENUE—		
In Bank, on Deposit Receipt, - - - - - -		£18 17 10
		£308 12 10

GLASGOW, 12th November, 1890.—Examined and found correct.

(Signed) THOMAS S. CREE.
JOHN FARQUHAR.

JNO. MANN, C.A., *Treasurer*.

THE SCIENCE LECTURES ASSOCIATION FUND.

Dr. ABSTRACT OF TREASURER'S ACCOUNT—SESSION 1889-90. Cr.

Dr.

CAPITAL AT 1ST NOVEMBER, 1889—		
£200 Caledonian Railway Company 4% Preference Stock, No. 1, in name of the Philosophical Society, in Trust, cost - - - - - - - - -	£244 4 8	
On Deposit Receipt, - - - - -	8 5 4	
		£252 10 0
Revenue of 1888-89, in Bank, - - - - -		10 8 3
REVENUE—		
Dividend, April, 1890, less Tax, - -	£3 18 0	
,, Oct., ,, ,, - -	3 18 0	
Interest from Bank, - - - - -	0 8 1	
		8 4 1
		£271 2 4

Cr.

PHILOSOPHICAL SOCIETY—Revenue of 1888-89—paid over as a grant towards Lecture on "Animal Locomotion" on 26th February, 1890, by Mr. Eadweard Muybridge; and Lecture on "Electrical Oscillation," by Professor Oliver J. Lodge, on 16th April, 1890, - - - - - - - - - -		£10 8 3
CAPITAL—		
Investment, *per contra*, - - - -	£244 4 8	
In Bank on Deposit Receipt, -	8 5 4	
		252 10 0
BALANCE, BEING REVENUE—		
In Bank on Deposit Receipt, - - - - - -		8 4 1
		£271 2 4

GLASGOW, 12th November, 1890.—Examined and found correct.

(Signed) THOMAS S. CREE.
JOHN FARQUHAR.

JNO. MANN, C.A., *Treasurer.*

5. Mr. John Robertson, on behalf of the Library Committee, submitted the following Report on the State of the Library. Its adoption was agreed to, and, on the motion of Mr. Robertson, the thanks of the Society were awarded to the donors of Books to the Library during the year:—

Report of the Library Committee.

During the past year 523 volumes were issued to 331 members. At present 100 periodicals are received at the Library, and of these 56 are bought and 44 presented. They form altogether 113 volumes annually.

The presentations to the Society during the year amounted to 23 volumes, 18 parts of works, and 29 pamphlets, while 28 volumes and 193 parts were received in exchange from 161 Societies and Public Departments, and 33 volumes were purchased. Altogether there has been a total addition to the Library of 197 volumes, 211 parts, and 29 pamphlets. There have been bound in the course of the year 258 volumes.

A careful scrutiny of the books has been made during the autumn. They now number 10,864.

Since last Report the following Societies have been added to the exchange list:—Geological and Natural History Society of Minnesota, Junior Engineering Society, London; American Society of Civil Engineers, American Museum of Natural History, North-East Coast Institution of Engineers and Shipbuilders.

In Volume XXI. of the *Proceedings*, pp. 257-265, there will be found a list of the additions to the Library by purchase up to June, 1890, the titles of the books presented, with the names of the donors, the names of the Societies and Public Departments with which exchanges are effected, and a list of the periodicals received by the Society.

JOHN ROBERTSON, Librarian,
Convener.

6. On the motion of the Chairman, the best thanks of the Society were awarded to the Treasurer and the Librarian for their services during the past year.

7. The Society then proceeded to the election of Office-Bearers:—

(1) On the recommendation of the Council, and on the motion of Dr. J. T. Bottomley, seconded by Professor Dittmar, Professor James Blyth, M.A., F.R.S.E., Glasgow and West of Scotland Technical College, was elected a Vice-President in succession to Dr. W. G. Blackie.

(2) On the motion of Mr. Adam Knox, the following gentlemen were elected members of Council for the full term of three years:—Mr. Walter W. Blackie, B.Sc.; Mr. John G. Kerr, M.A., E.C. Normal Training College; Dr. Joseph Coats, and Mr. Charles A. Fawsitt, in room of Mr. Knox, Dr. Thomas Muir, Dr. John Glaister, and Mr. D. Sinclair, whose term of office had expired; and Rev. Alexander Brunton was elected for two years in room of Professor Blyth.

(3) On the motion of Dr. Turner, seconded by Mr. Alexander Scott, the Office-Bearers of the Geographical and Ethnological Section were elected, in accordance with resolution of Society of 11th April, 1883. There were also elected the Office-Bearers of the Sanitary and Social Economy, Mathematical and Physical, and Economic Science Sections, according to Resolution of Society of 18th November, 1885, and 2nd February, 1887; and on the motion of Dr. Colville, seconded by Dr. Turner, the List of Office-Bearers of the Philological Section was agreed to. (Lists of Office-Bearers of the Society and of the various Sections will be found on pp. 340-344.)

(4) On the motion of the Chairman, the Librarian, Treasurer, and Secretary were re-elected to their respective offices.

8. A Paper on "A Problem in Ventilation by Heat" (Experimentally Illustrated) was read by Mr. W. P. Buchan, sanitary engineer. Some discussion took place, in which the speakers were Professor Blyth, Mr. W. R. W. Smith, Dr. J. T. Bottomley, and Mr. Arthur Mechan. Mr. Buchan replied, and was awarded the thanks of the Society for his paper and demonstration.

9. The President exhibited and described a new form of Electric Contact Breaker for Physiological Experiments, and gave a Demonstration of Tetanus.

10. The President also exhibited and explained a series of Chronological Tables of Scientific Men (more especially of Anatomists and Physiologists), and of their most distinguished contemporaries—compiled by himself. After some remarks had been made on the subject of the Chronological Tables, the thanks of the Society were awarded to the President, on the motion of Dr. Bottomley, who had temporarily taken the Chair.

11. A Paper, entitled "Additional Remarks on the First Editions of the Chemical Writings of Democritus and Synesius," was read by Professor John Ferguson, M.A., LL.D. (Supplementary to Paper read in November, 1884, and published in Vol. XVI. of the Society's *Proceedings*.) Professor Ferguson received the thanks of the Society for his communication.

12. The President announced that the following Candidates for Membership of the Society had all been elected:—Mr. William Brooks Sayers, electrical engineer, 56 George Square; Mr. George Ritchie, analytical chemist, Parkhead Forge and Steel Works; Mr. Robert Duncan, engineer, Whitefield Works, Govan; Mr.

Robert Nisbet, ironfounder, Star Foundry, Kinning Park; Mr. James R. Garrow, chemist, Irvine (246 Bath Street, Glasgow); Mr. Alex. Leckie Wright, analytical chemist, Coats Iron and Steel Works, Coatbridge; Mr. Robert Ludwig Mond, B.A. (Cantab.), F.R.S.E.; Dr. William Snodgrass, M.A., Muirhead Demonstrator of Physiology, University of Glasgow.

10th December, 1890.

The Second Ordinary Meeting of the Philosophical Society of Glasgow for Session 1890-91 (postponed from 3rd inst.), was held in the Society's Rooms, 207 Bath Street, on the Evening of Wednesday, 10th December, 1890, at Eight o'Clock—Dr. J. G. M'Kendrick, F.R.S., President, in the Chair.

1. The Minutes of the Annual General Meeting, which were printed in the Billet calling the Meeting, were held as read, were approved of, and signed by the Chairman.

2. The following gentlemen, elected on 19th November, were admitted to the Membership of the Society:—Mr. William Brooks Sayers, Mr. George Ritchie, Mr. Robert Duncan, Mr. Robert Nisbet, Mr. James R. Garrow, Mr. Alex. Leckie Wright, Mr. Robert Ludwig Mond, B.A. (Cantab.), F.R.S.E., and Dr. William Snodgrass, M.A.

3. At the suggestion of the Chairman, it was agreed to suspend the Standing Orders, in order that he might submit a resolution which had already been approved of at a Meeting of the Council. The resolution, he said, had reference to a distinguished member of the Society—Sir William Thomson. Sir William had just been called to occupy the Chair of the Royal Society, an appointment which might be regarded as the crowning honour of a very distinguished career. It would be out of place for him to enlarge before the Society upon Sir William Thomson's attainments in science, or upon his many estimable qualities as an individual, because he was perhaps better known there than he was anywhere else; although, certainly, there was no member of their Society, and no citizen of Glasgow, who was so well known over the whole civilised world. The resolution was as follows:—

"That the Philosophical Society of Glasgow desires to convey to Sir William Thomson its congratulations on his election to the Presidentship of

the Royal Society. The members of the Society cannot forget the warm interest that Sir William has always shown in the Philosophical Society, and they feel proud that one who has been a member since 1846, and who has been twice its President, has been called upon to occupy the chair of the leading scientific society in the world. They feel sure that the accession to this office by Sir William Thomson will be hailed with acclamation by all scientific men as a just tribute, not only to his eminence as a mathematician, physicist, and electrician, but also to his many estimable qualities as a man."

Dr. Charles Gairdner seconded the resolution, which was unanimously agreed to.

4. Dr. M'Kendrick having vacated the Chair in favour of Dr. Gairdner, President of the Economic Science Section, Dr. James Bonar, M.A., London, author of "Malthus and his Work," &c., proceeded to read a paper on "Carlyle and Political Economy." *(A Communication from the Economic Science Section.)* Afterwards a discussion took place, in which the speakers were Mr. Wm. Smart, Mr. Cree, Dr. H. Dyer, Mr. George Younger, Mr. Robert Duncan, Mr. Ewen, and the Chairman, on whose motion the thanks of the Society were awarded to Dr. Bonar, who acknowledged the vote of thanks and briefly replied to the speakers.

5. The Chairman announced that the following Candidates for Membership of the Society had all been elected:—Mr. R. C. Fulton, master plumber, 2 Lugar Place, Kelvinside; Mr. James A. Napier, manufacturer, 55 Cathedral Street; Mr. William Costigane, warehouseman, Clifton House, Pollokshields; Mr. W. B. Faulds, writer, Westfield, Ibrox; Mr. James Kirkwood, stockbroker, Carling Lodge, Ibrox; Mr. William Anderson, R.P., 284 Buchanan Street; Mr. James Anderson, master plumber, 168 George Street; Mr. William A. Rattray, master plumber, 233 Hope Street; Mr. J. L. Arnot, sanitary engineer, 116 West Campbell Street.

17th December, 1890.

The Third Ordinary Meeting of the Philosophical Society of Glasgow for Session 1890-91 was held in the Society's Rooms, 207 Bath Street, on the Evening of Wednesday, 17th December, 1890, at Eight o'Clock. In the absence of the President and Vice-Presidents, Dr. James Colville, Member of Council, occupied the Chair.

1. The Minutes of the Second Ordinary Meeting of the Society, which were printed in the Billet calling the Meeting, were held as read, were approved of, and signed by the Chairman.

2. The following gentlemen, elected on 10th December, were admitted to the Membership of the Society:—Mr. R. C. Fulton, Mr. James A. Napier, Mr. William Costigane, Mr. W. B. Faulds, Mr. James Kirkwood, Mr. William Anderson, Mr. James Anderson, Mr. William A. Rattray, and Mr. J. L. Arnot.

3. The Secretary, referring to the resolution passed at the previous Meeting congratulating Sir William Thomson on his election to the Presidentship of the Royal Society, read the following letter which he had received from Sir William:—

PHYSICAL LABORATORY, THE UNIVERSITY,
GLASGOW, *December 16, 1890.*

DEAR MR. MAYER,

On my return from London I received your letter of the 10th, with copy of a Resolution adopted by the Philosophical Society at its meeting of last week.

Will you convey to the Society my warmest thanks for their kindness in thus congratulating me on my election to the Presidentship of the Royal Society, and in their friendly recollection of the times during which I have been their own President; and tell them that I always feel those old times—and not so old times—among my happiest recollections.

With thanks to yourself personally too for your kind words,

I remain,

Yours very truly,

WILLIAM THOMSON.

4. Professor Dittmar, F.R.S., gave a summary of a paper by himself and Mr. J. B. Henderson on "The Gravimetric Composition of Water," for which he received the thanks of the Society.

5. Mr. James Erskine Murray, Physical Laboratory, University of Glasgow, read a paper "On the Viscosity of Air," in which were given the results of a recent experimental investigation on that subject. He was awarded the thanks of the Society for his paper.

6. Mr. William Lang, jun., F.C.S., President of the Glasgow Photographic Association, exhibited to the Meeting a small book entitled "Giphantie," and gave from it a remarkable forecast of photography made in 1760. The thanks of the Meeting were awarded to Mr. Lang for his interesting communication.

7. The Chairman announced that the following Candidates for Membership of the Society had all been elected:—Mr. John Summers, R.P., master plumber, 174 West Nile Street; Mr. David Fulton, R.P., sanitary engineer, Roxburgh Villa, Bothwell; Mr. W. E. H. Wood, master plumber, 40 Candleriggs; Mr. P. MacGregor Chalmers, F.S.A.Scot., architect; Mr. Robert M'Callum, jun., house factor, Union Street; Mr. James D. Borthwick, Treasurer of Police, 8 Broomhill Terrace.

7th January, 1891.

The Fourth Ordinary Meeting of the Philosophical Society of Glasgow for Session 1890-91 was held in the Society's Rooms, 207 Bath Street, on the Evening of Wednesday, 7th January, 1891, at Eight o'Clock—Dr. J. G. M'Kendrick, F.R.S., President, in the Chair.

1. The Minutes of the Third Ordinary Meeting of the Society, which were printed in the Billet calling the Meeting, were held as read, were approved of, and signed by the Chairman.

2. The following gentlemen, elected on 17th December, were admitted to the membership of the Society:—Mr. John Summers, Mr. David Fulton, Mr. W. E. H. Wood, Mr. P. MacGregor Chalmers, F.S.A.Scot., Mr. Robert M'Callum, jun., and Mr. James D. Borthwick.

3. A paper on "The Oyster Fishery of Scotland" was read by J. H. Fullarton, M.A., D.Sc., member of the scientific staff of the Scottish Fishery Board, Marine Station, Dunbar. Some remarks were made on the subject of the paper by Mr. Goodwin, Mr. W. R. W. Smith, and the Chairman, and Dr. Fullarton was awarded the thanks of the Society for his communication.

4. Professor Blyth, M.A., F.R.S.E., gave a description of "A Combined Windmill and Dynamo" which he had brought into practical use, and which he illustrated by means of a small model. A discussion followed, in which the speakers were Mr. Samuel Mavor, Mr. W. R. W. Smith, Mr. W. C. Martin, and the Chairman. The thanks of the Society were awarded to Professor Blyth for his communication.

5. In the absence of Mr. A. Erskine Muirhead, who was detained in Paris, his description of the new French Telephones was held over for a subsequent meeting.

6. The Chairman announced that the following Candidate for membership of the Society had been unanimously elected:—Mr. Charles S. Swan, mercantile clerk, 15 Rose Street, Garnethill.

21st January, 1891.

The Fifth Ordinary Meeting of the Philosophical Society of Glasgow for Session 1890-91 was held in the Society's Rooms, 207 Bath Street, on the Evening of Wednesday, 21st January, 1891, at Eight o'Clock—Dr. J. G. M'Kendrick, F.R.S., President, in the Chair.

1. The Minutes of the Fourth Ordinary Meeting of the Society, which were printed in the Billet calling the Meeting, were held as read, were approved of, and signed by the Chairman.

2. Mr. Charles C. Swan, elected on 7th January, was admitted to the Membership of the Society.

3. Professor Max Müller, M.A., of the University of Oxford, and an Honorary Member of the Society, read a paper "On Language," which was listened to with great pleasure by a large audience, and for which, on the motion of Dr. Colville, he was awarded the best thanks of the Society.

4th February, 1891.

The Sixth Ordinary Meeting of the Philosophical Society of Glasgow for Session 1890-91 was held in the Society's Rooms, 207 Bath Street, on the Evening of Wednesday, 4th February, 1891, at Eight o'Clock—Dr. J. G. M'Kendrick, F.R.S., President, in the Chair.

1. The Minutes of the Fifth Ordinary Meeting of the Society, which were printed in the Billet calling the Meeting, were held as read, were approved of, and signed by the Chairman.

2. Professor James Mavor read a paper on "Iceland: Some Economic and other Notes," which was illustrated by an extensive series of Lantern Views of Icelandic Scenery. The author was awarded a very hearty vote of thanks for his paper and the illustrations of the same.

3. Mr. A. Erskine Muirhead exhibited and briefly described the latest forms of French Telephones, including such as are specially adapted for Railway Signal Cabins, Steamers, and Mines, or for Military or other temporary purposes. After some remarks by Professor Blyth, and Messrs. Aitken, Goodwin, W. C. Martin, and Sayers, to which Mr. Muirhead replied, he was thanked by the Meeting for his communication.

The Chairman announced that the following Candidates for Membership had all been elected:—Mr. Geo. Barclay, merchant, 63 St. Vincent Street; Mr. David Johnston, writer, 100 West George Street; Mr. John Scott, electrical engineer, 140 Douglas Street; Mr. Harry Darwin, gas engineer, St. Andrew's Works, 618 Eglinton Street; Mr. John Guthrie, engineer, 465 Eglinton Street.

18th February, 1891.

The Seventh Ordinary Meeting of the Philosophical Society of Glasgow for Session 1890-91 was held in the Society's Rooms, 207 Bath Street, on the Evening of Wednesday, 18th February, 1891, at Eight o'Clock—Dr. J. G. M'Kendrick, F.R.S., President, in the Chair.

1. The Minutes of the Sixth Ordinary Meeting of the Society, which were printed in the Billet calling the Meeting, were held as read, were approved of, and signed by the Chairman.

2. The following gentlemen, elected on 4th February, were admitted to the Membership of the Society:—Mr. George Barclay, Mr. David Johnstone, Mr. John Scott, Mr. Harry Darwin, and Mr. John Guthrie.

3. Mr. W. P. Buchan, sanitary engineer, delivered his Address as President of the Sanitary and Social Economy Section, its subject being "On the Progress of Sanitation, with special reference to the Sanitary Condition of the Glasgow Public

Schools." With the permission of Mr. Buchan the address was left open for discussion—the speakers being Mr. William Bathgate, M.A.; Dr. Eben. Duncan, Mr. Gilbert Thomson, C.E., M.A.; Mr. Wm. Key, Mr. David Thomson, and the President. Mr. Buchan was heartily thanked for his address.

4. Mr. Magnus Maclean, M.A., F.R.S.E., of the Physical Laboratory, University of Glasgow, made a short communication to the Society, by himself and Mr. Makita Goto, on "Some Electrical Properties of Flames." After some remarks by Mr. Mond, the thanks of the Society were awarded to Mr. Maclean.

4th March, 1891.

The Eighth Ordinary Meeting of the Philosophical Society of Glasgow for Session 1890-91 was held in the Society's Rooms, 207 Bath Street, on the Evening of Wednesday, 4th March, 1891, at Eight o'Clock—Dr. J. T. Bottomley, F.R.S., Vice-President, in the Chair.

1. The Minutes of the Seventh Ordinary Meeting of the Society, which were printed in the Billet calling the Meeting, were held as read, were approved of, and signed by the Chairman.

2. Mr. Mayer, Secretary, read a paper by Mr. John Struthers, London, on "A New Railway Scheme for Scotland (including New Main Line), and its possible Developments." After some remarks had been made on the Scheme by Mr. Alexander George Moore, C.E., the thanks of the Society were passed to Mr. Struthers.

3. Mr. Robert Ludwig Mond, B.A. (Cantab.), F.R.S.E., read a paper entitled "Some Suggestions towards a New Theory of Electrolysis," on which some remarks were made by the Chairman, Mr. Magnus Maclean, Mr. Meikle, and Mr. Sam. Mavor. The author was awarded the thanks of the Society.

4. The Chairman announced that the following Candidates for Membership of the Society had been unanimously elected:—Mr. J. Robertson Watson, M.A., Professor of Chemistry in Anderson's College Medical School; Mr. Charles L. Spencer, Edgehill, Kelvinside.

18th March, 1891.

The Ninth Ordinary Meeting of the Philosophical Society of Glasgow for Session 1890-91 was held in the Society's Rooms, 207 Bath Street, on the Evening of Wednesday, 18th March, 1891, at Eight o'Clock—Dr. J. G. M'Kendrick, F.R.S., President, in the Chair.

1. The Minutes of the Eighth Ordinary Meeting of the Society, which were printed in the Billet calling the Meeting, were held as read, were approved of, and signed by the Chairman.

2. Professor J. Robertson Watson, M.A., and Mr. Charles L. Spencer, were admitted to the Membership of the Society.

3. Professor Barr, D.Sc., M.Inst.C.E., gave a Lecture and Demonstration on "The use of the Optical Lantern for Class-Room Work. (*a*) The use of the Lantern in Lighted Rooms. (*b*) A New Lantern specially suited for Scientific Demonstration. (*c*) A new Apparatus for the Preparation of Lantern Slides." Remarks were made on the subject by the Chairman, Sir James Marwick, Dr. Muir, and Dr. Fleming; and on the motion of the Chairman, a cordial vote of thanks was awarded to Professor Barr for his interesting communication.

3. Dr. J. T. Bottomley, M.A., F.R.S., showed and gave a brief description of Mr. John Aitken's New Portable Dust Counter, for which he was awarded the best thanks of the Society.

1st April, 1891.

The Tenth Ordinary Meeting of the Philosophical Society of Glasgow for Session 1890-91 was held in the Society's Rooms, 207 Bath Street, on the Evening of Wednesday, 1st April, 1891, at Eight o'Clock—Dr. J. G. M'Kendrick, F.R.S., President, in the Chair.

1. The Minutes of the Ninth Ordinary Meeting of the Society, which were printed in the Billet calling the Meeting, were held as read, were approved of, and signed by the Chairman.

2. A paper on "An Inquiry into the Nature of Heredity" was read by Dr. William Wallace, M.A. Some remarks were made

on the paper by the President, and questions were asked by Messrs. H. A. Mavor and W. P. Buchan. The author briefly replied, and was awarded a cordial vote of thanks for his paper.

3. Professor Blyth, M.A., F.R.S.E., submitted "An Experiment on Self-Induction," and made a brief explanatory statement regarding it, for which he was voted the best thanks of the Meeting.

15th April, 1891.

The Eleventh Ordinary Meeting of the Philosophical Society of Glasgow for Session 1890-91 was held in the Society's Rooms, 207 Bath Street, on the Evening of Wednesday, 15th April, 1891, at Eight o'Clock—Dr. J. T. Bottomley, F.R.S., Vice-President, in the Chair.

1. The Minutes of the Tenth Ordinary Meeting of the Society, which were printed in the Billet calling the Meeting, were held as read, were approved of, and signed by the Chairman.

2. The Secretary read a short Biographical Notice of the late Mr. Alexander Whitelaw, a former member of Council, for which he was awarded the thanks of the Society.

3. Mr. Dugald Bell, a former Vice-President of the Geological Society of Glasgow, read a paper entitled "The Great Winter: a Chapter in Geology." Some remarks were made on the subject of the paper by the Chairman, Mr. William Jolly, F.G.S.; Mr. John Young, F.G.S.; Mr. James White, and Mr. W. P. Buchan; and the best thanks of the Society were awarded to Mr. Bell.

29th April, 1891.

The Twelfth Ordinary and Closing Meeting of the Philosophical Society of Glasgow, for Session 1890-91, was held in the Rooms of the Society, 207 Bath Street, on the Evening of Wednesday, 29th April, 1891, at Eight o'clock—Dr. J. G. M'Kendrick, F.R.S., President, in the Chair.

1. The Minutes of the Eleventh Ordinary Meeting of the Society, which were printed in the Billet calling the Meeting, were held as read, were approved of, and signed by the Chairman.

2. Dr. Alexander Buchan, M.A., F.R.S.E., Secretary of the Scottish Meteorological Society, read a paper on "The Meteorological Results of the 'Challenger' Expedition in relation to Physical Geography and Geology." Some remarks were made on the subject of the paper by Dr. Thomas Muir, after which the thanks of the Society were awarded to Dr. Buchan, on the motion of the President.

3. Dr. Charles Annandale, M.A., read a paper on "The Poem of Béowulf, in the light of Recent Studies." Dr. Colville briefly spoke on the paper, and the author was awarded the thanks of the Society for his communication.

4. A short communication was submitted by Dr. Snodgrass, from the President and himself, on "The Physiological Action of Carbon-monoxide of Nickel [$Ni\ (CO)_4$]." Mr. R. L. Mond made a brief statement on the subject, referring to some further investigations in his father's laboratory, where the compound had been discovered. The authors were thanked for their communication, and, on the motion of Mr. Alexander Scott, a special vote of thanks was given to the President for his services during the Session.

5. The Reports of Sections for Session 1890-91 were presented by the Secretary, and ordered to be printed in the next volume of the *Proceedings.*

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Nitrogen: its Uses and Sources in Agriculture. By C. M. Aikman. Pamphlet. 1890, . .	,,
Report on the Operations of the Sanitary Department of the City of Glasgow. By Peter Fyfe. Pamphlet. 1889, ...	,,
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Annual Report of the Chief Signal Officer, U.S. Army, for 1889. Parts I. and II. 8vo. Washington, 1890, ...	The Chief Officer.
Monographs of the United States Geological Survey. Vol. 15. Parts I., II. Potomac or Younger Mesozoic Flora. By W. M. Fontaine. 4to. Washington, 1889, ...	U.S. Government.
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Dictionary of the Language of the Micmac Indians. By Rev. S. T. Rand, D.D., LL.D. 4to. Halifax, N.S., 1888, ...	Geo. E. Foster.

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Indications of Retrogression in Pre-historic Civilisation in the Thames Valley. By H. Stopes. Pamphlet. 1890, ...	British Association.
Decimal Coinage: Weights and Measures Popularly Explained. By Sir Guilford Molesworth and Emerson Dowson. Pamphlet. 1890, .	The Decimal Association.
Report on the Greening of French Vegetables with Sulphate of Copper. Pamphlet. 1890, .	The Glasgow Police Commissioners.
The Í Hing; or, "Patriotic Rising:" a Secret Society among the Chinese in America. (From Proceedings of the Numismatic and Antiquarian Society of Philadelphia, 1887-89.) By Stewart Culin. Pamphlet. 1890, . .	The Author.
Chinese Secret Societies in the United States. (From *Journal of American Folk-lore*, February-March, 1890.) By Stewart Culin. Pamphlet. 1890, ...	,,
Customs of the Chinese in America. (From the *Journal of American Folk-lore*, February-March, 1890.) By Stewart Culin. Pamphlet. 1890, ...	,,
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A Text-Book to the Art of Ventilating Buildings. By W. P. Buchan. 8vo. London, 1891, .	The Author.
Fossil Plants: Similkameen Valley and other places in British Columbia. By Sir J. W. Dawson. 4to Pamphlet. 1890, . . .	,,
Observations made at the Magnetical and Meteorological Observatory at Batavia. Vol. XII., 1889. Folio. 1890, ...	The Government of Batavia.
Rainfall in the East Indian Archipelago for 1889. 8vo. Batavia, 1890, ...	,,
Meteorological Results of the "Challenger" Expedition in relation to Physical Geography. By Alexander Buchan, M.A., LL.D., F.R.S.E. 8vo Pamphlet. 1891, ...	The Author.
Isothermals Isobars and Prevailing Winds of the Globe. By Alexander Buchan. (From Report of the Scientific Results of the Voyage of H.M.S. "Challenger.") Maps. 4to. No date,	,,

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Journal of the Photographic Society of London. Vols. VIII., IX., X., XII., 1862-68. 8vo. London, ...	,,
Platinotype. By Captain Pizzighelli and Baron A. Hubl. Translated by J. F. Iselin, and Edited by Captain W. de W. Abney. (Reprinted from the *Photographic Journal*, 1883.) 12mo. London, 1886, ...	,,
Kelvingrove Museum and Corporation Art Galleries of Glasgow. Report for 1890, . .	The Curator.
Report of the British Association Meeting at Leeds. 1890, ...	The Association.
Time: Reckoning for the 20th Century. By Sanford Fleming, C. M. G., LL. D., C.E. (From Smithsonian Report for 1886.) Pamphlet. Washington, 1889, ...	The Author.
Manurial Experiments with Turnips. By C. M. Aikman, M.A., B.Sc., F.R.S.E. Pamphlet. 1891, ...	,,
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A Report on the Proposed Congo Railway. By Colonel the Honourable G. W. Williams. Pamphlet, 1890, ...	,,
The Chemical Effects of the Spectrum. By Dr. J. M. Eder. Translated and Edited by Captain W. de W. Abney. (Reprint from *Photographic Journal*, 1881-82.) 12mo. London, 1883, ...	Wm. Lang, jun.
Reprints of three Editorials regarding the priority in demonstrating the Toxic Effect of Matter accompanying the Tubercle Bacillus and its Nidus. 12mo. No place nor date, . .	Academy of Natural Sciences of Philadelphia.
The Collection of Ancient Marbles at Leeds. By E. L. Hicks. (Reprinted from Journal of Hellenic Studies, Vol. XI.) Sm. 4to Pamphlet. 1890, ...	Leeds Philosophical and Literary Society.

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Report of Observations made by the Washington University Eclipse Party at Norman, California, January 1st, 1889. 4to. Cambridge, 1891, ...	St. Louis Academy of Science.

BOOKS PURCHASED 1890-91.

Dictionary of National Biography. Vols. 23, 24, 25, 26, 27.

In Darkest Africa. By H. M. Stanley. 2 vols. 8vo. London, 1890.

A Monograph of the British Phytophagous Hymenoptera. Vol. 3. By Peter Cameron. 8vo. London, 1890.

Association Française pour l'Avancement des Sciences. 1889 and 1890.

Text-Book of Algebra for the Higher Classes of Secondary Schools and Colleges. 2 vols. 2nd edition. By G. Chrystal. 8vo. Edinburgh, 1889.

Mathematical Papers of Arthur Cayley. Vol. 3. 4to. Cambridge, 1890.

Repertorium der Technischen Journal-Litteratur, for 1889. By Dr. Rieth. Berlin, 1890.

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Hazell's Annual, 1891.

Zoological Record. Vol. 26. 1889.

La Nature. Vol. 18.

Œuvres Complètes. De Laplace. Tome 8. 4to. Paris, 1891.

Larvæ of the British Butterflies and Moths. By (the late) William Buckler. Edited by H. T. Stainton. Vol. 4. 8vo. London, 1890.

Official Year Book of the Scientific and Learned Societies. 1891.

Journal of Iron and Steel Institute. Parts 1 and 2. 1890.

History of Chemistry. By Ernst Von Meyer. Translated by George M'Gowan. 8vo. London, 1891.

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The Theory of Light. By Thomas Preston. 8vo. London, 1890.

Handbook and Atlas of Astronomy. By W. Peck. 4to. London, 1890.

Palæontographical Society's Publications—

- The Stromatoporoids. Part 3. By H. A. Nicholson. 6 Plates.
- The British Fossil Echinodermata. By W. P. Sladen. Part 1. 8 Plates.
- The Inferior Oolite Ammonites of the British Isles. By S. S. Buckman. Part 5. 8 Plates.
- The Devonian Fauna of the South of England. Part 3. By G. F. Whidborne. 9 Plates.

Men and Women of the Time: a Dictionary of Contemporaries. By G. Washington Moon. 13th Edition. 8vo. London, 1891.

Manual of Palæontology for the use of Students. 2 vols. 3rd Edition. By Henry A. Nicholson and R. Lydekker. 8vo. Edinburgh and London, 1889.

Distribution of Rain over the British Isles during the years 1880 to 1884. 5 vols. By G. J. Symons. 8vo. London, 1881-85.

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Planetary and Stellar Studies. By John E. Gore. 8vo. London, 1888.
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Coloured Figures of the Birds of the British Islands. By Lord Lilford. 2nd Edition. 8vo. London, 1891.
History of Philosophy. By George H. Lewes. 2 vols. 8vo. London, 1885.

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Builder.
Building News.
Chemical News.
Comptes Rendus.
Dingler's Polytechnisches Journal.
Economist.
Electrician.
Engineer.
Engineering.
English Mechanic.
Industries.
Iron.
Journal of the Society of Arts.
Journal of Gas Lighting, &c.
Lancet.
Nature.
Notes and Queries.
Pharmaceutical Journal.
Publishers' Circular.
Science.
Scientific American and Supplement.
Telegraph Journal and Electrical Review.

FORTNIGHTLY.

Annalen der Chemie (Liebig's).
Berichte der Deutschen Chemischen Gesellschaft.
Journal für Praktische Chemie (Erdmann's).
Zeitschrift für Angewandte Chemie.

MONTHLY.

American Chemical Journal.
American Journal of Science.
Analyst.
Annalen der Physik und Chemie.
Annales de Chimie et de Physique.
Annales de l'Institut Pasteur.
Annales des Ponts et des Chaussées.
Annales des Sciences Naturelles—Botanique.
Annales des Sciences Naturelles—Zoologie.
Annals and Magazine of Natural History.
Antiquary.
Beiblätter zu den Annalen der Physik und Chemie.
Boletin Mensuel d'Observatorio Meteorologico-Magnetico Central de Mexico.
Bookseller.
Bulletin de la Société d'Encouragement.

Bulletin de la Société Géologique de France.
Bulletin de la Société Industrielle de Mulhouse.
Bulletin Mensuel de l'Observatoire de Montsouris.
Canadian Entomologist.
Chamber of Commerce Journal.
Deutsche Kolonialzeitung.
Economic Journal.
Entomologist.
Entomologists'. Monthly Magazine.
Geological Magazine.
Hardwicke's Science Gossip.
Johns Hopkins University Circulars.
Journal de Pharmacie et de Chimie.
Journal of Botany.
Journal of the Chemical Society.
Journal of the Franklin Institute.
Journal of the Photographic Society.
Journal of Society of Chemical Industry.
London, Edinburgh, and Dublin Philosophical Magazine.
Midland Naturalist.
Monatsbericht der Königlich Preussischen Akademie der Wissenschaften zu Berlin.
Petermann's Mitteilungen.
Polytechnic Bibliothek.
Proceedings of Royal Geographical Society.
Proceedings of Royal Society of London.
Proceedings of the Society of Biblical Archæology.
Revue Universelle des Mines.
Royal Astronomical Society's Monthly Notices.
Sanitary Journal.
Scottish Geographical Magazine.
Zoologist.

QUARTERLY.

Annales des Mines.
Annals of Botany.
Archives Nèerlandaises des Sciences Exactes et Naturelles.
Bulletin of the American Geographical Society.
Forschritte der Mathematik.
Grevillea.
Ibis.
Journal of Anatomy and Physiology.
Journal of the Anthropological Institute of Great Britain.
Journal of Manchester Geographical Society.
Journal of the Royal Agricultural Society of England.
Journal of the Royal Statistical Society.
Journal of the Scottish Meteorological Society.
La Nature.
Mind: a Quarterly Review of Psychology and Philosophy.
Quarterly Journal of Economics.
Quarterly Journal of Geological Society.
Quarterly Journal of Microscopical Science.
Quarterly Journal of Pure and Applied Mathematics.
School of Mines Quarterly.
Scottish Naturalist.
Sociedad Cientifica "Antonio Alzate."
Zeitschrift für Analytische Chemie.

LIST OF MEMBERS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW,

FOR 1890-91.

HONORARY MEMBERS.

(Limited to Twenty.)

WITH YEAR OF ELECTION.

FOREIGN.

	Hermann Ludwig Ferdinand von Helmholtz, Berlin.	1860
	Rudolph Albert von Kölliker, Würtzburg.	1860
	Ernst Heinrich Hæckel, Jena.	1880
	Louis Pasteur, Paris.	1885
5	Georg Quincke, Heidelberg.	1890

AMERICAN AND COLONIAL.

	James Dwight Dana, LL.D., Professor of Geology and Mineralogy in Yale College, Connecticut.	1860
	Robert Lewis John Ellery, F.R.A.S., Victoria.	1874
	Sir John William Dawson, LL.D., F.R.S., Principal of Macgill College, Montreal.	1883

BRITISH.

	Sir Andrew Crombie Ramsay, LL.D., F.R.S., London.	1874
10	Sir Joseph Dalton Hooker, K.C.B., K.C.S.I., M.D., D.C.L., LL.D., F.R.S., Kew.	1874
	Thomas Henry Huxley, Ph.D., LL.D., D.C.L., F.R.S., London.	1876
	Herbert Spencer, London.	1879
	John Tyndall, LL.D., D.C.L., F.R.S., M.R.I., London,	1880
	Rev. John Kerr, LL.D., Glasgow.	1885
15	Sir George Gabriel Stokes, Bart., M.A., LL.D., D.C.L., *Pres.*R.S., M.P., Cambridge.	1887
	F. Max Müller, M.A., Professor of Comparative Philology, Oxford.	1889
	The Right Hon. Lord Rayleigh, M.A., D.C.L., LL.D., *Sec.*R.S., London.	1890

CORRESPONDING MEMBERS.

WITH YEAR OF ELECTION.

Rev. H. W. Crosskey, LL.D., F.G.S., 117 Gough road, Birmingham. 1874
A. S. Herschel, M.A., D.C.L., F.R.S., F.R.A.S., Hon. Professor of Experimental Physics in the Durham College of Science, Newcastle-on-Tyne; Observatory House, Slough, Bucks. 1874
Thomas E. Thorpe, Ph.D., F.R.S., Royal School of Mines, London. . . . 1874
John Aitken, F.R.S., F.R.S.E., Darroch, Falkirk. 1883
5 Alex. Buchan, M.A., LL.D., F.R.S.E., Secretary to the Scottish Meteorological Society, 73 Northumberland street, Edinburgh. 1883
James Dewar, M.A., F.R.S., F.R.S.E., M.R.I., Jacksonian Professor of Physics, University of Cambridge, and Professor of Chemistry in the Royal Institution of Great Britain. 1883
Stevenson Macadam, Ph.D., F.R.S.E., Lecturer on Chemistry, Surgeons' Hall, Edinburgh. 1883
Joseph W. Swan, M.A., Newcastle-on-Tyne and London. 1883
E. A. Wünsch, F.G.S., London. 1883
10 George Anderson, Master of the Mint, Melbourne. 1885

ORDINARY MEMBERS.

WITH YEAR OF ENTRY.

* *Denotes Life Members.*

Ackroyd, J. E., 7 Campbell street, Maryhill. 1887
Adam, William, M.A., 235 Bath st. 1876
Addie, John, 144 St. Vincent street. 1861
Aikman, C. M., M.A., B.Sc., F.R.S.E., F.I.C., F.C.S., Lecturer on Agricultural Chemistry, Technical College, 183 St. Vincent street. 1886
5 Aitken, William, National Telephone Co. 1890
Alexander, D. M., 8 Royal crescent, Crosshill. 1887
Alexander, Peter, M.A., 26 Smith street, Hillhead. 1885
Alexander, Thos., 48 Sardinia ter. 1869
Alexander, William, Helen street, Govan. 1889
10 Alley, Stephen, Sentinel Works, Polmadie road. 1884
Alstón, J. Carfrae, 9 Lorraine gardens, Dowanhill. 1887
Amours, F. J., B.A., High School. 1888
Anderson, Alexander, 157 Trongate. 1869
Anderson, James, 168 George street. 1890
15 Anderson, John, 22 Ann street. 1884
Anderson, Robert, jun., 22 Ann street. 1887
Anderson, R. T. R., 618 Gallowgate street. 1889
*Anderson, T. M'Call, M.D., Professor of Clinical Medicine in the University of Glasgow, 2 Woodside terrace. 1873
*Anderson, William, 284 Buchanan street. 1890
Anderson, W. F. G., 47 Union street. 1878
Annan, J. Craig, 153 Sauchiehall st. 1888
Annandale, Charles, M.A., LL.D., 86 Dixon avenue, Crosshill. 1888
Arnot, James Craig, 162 St. Vincent street. 1869
Arnot, J. L., 116 West Campbell st. 1890
25 Arrol, Walter, 16 Dixon street. 1869
Arrol, William A., 16 Dixon street. 1869
Atkinson, J. B., 10 Foremount terrace, Partick. 1889

Bain, Andrew, 17 Athole gardens. 1890
Bain, Sir James, F.R.S.E., 3 Park terrace. 1866
30 Bain, Robert, 132 West Nile street. 1869
Balloch, Robert, 131 St. Vincent st. 1843
Balmain, Thos., 1 Kew terrace, Kelvinside. 1881
Barclay, A. P., 63 St. Vincent street. 1890
Barclay, George, 63 St. Vincent st. 1891
35 Barclay, James, 36 Windsor terrace. 1871
Barlow, John, M.D., Professor of Physiology, St. Mungo's College, 8 Elmbank crescent. 1880
Barrett, Francis Thornton, Mitchell Library. 1880
Barr, Archibald, D.Sc., Professor of Civil Engineering and Mechanics, University of Glasgow, Royston, Dowanhill. 1890
*Barr, James, C.E., I.M., 221 West George street. 1883
40 Barr, Thos., M.D., F.F.P.S.G., 13 Woodside place, W. 1879
Bathgate, William, M.A., 13 Westbourne gardens. 1887

Bayne, A. Malloch, 1 Hamilton terrace, Partick. 1878
Beatson, George T., B.A. Cantab., M.D., 2 Royal crescent. 1881
Begg, Wm., 636 Springfield road. 1883
45 *Beith, Gilbert, 7 Royal Bank place. 1881
Bell, Dugald, 27 Lansdowne cres. 1871
*Bell, Henry, 5 Cornwall terrace, Regent's Park, London, N.W. 1876
Bell, James, 7 Marlborough terrace, Kelvinside. 1877
Bennett, Robert J., Alloway park, Ayr. 1883
50 Bilsland, William, 28 Park circus. 1888
Binnie, J., 69 Bath street. 1877
Binnie, Robert, Ashbourne, Gourock. 1881
Black, Adam Elliot, C.A., F.C.S., 5 Hillsborough square, Bruce st., Hillhead. 1880
Black, D. Campbell, M.D., M.R.C.S.E., 121 Douglas street. 1872
55 Black, J. Albert, Duneira, Row. 1869
Black, John, 16 Park terrace. 1869
Black, Malcolm, M.B., C.M., 5 Canning place. 1880
*Blackie, J. Alexander, 17 Stanhope street. 1881
*Blackie, J. Robertson, 17 Stanhope street. 1881
60 Blackie, Robert, 17 Stanhope st. 1847
Blackie, W. G., Ph.D., LL.D., F.R.G.S., 17 Stanhope street. 1841
Blackie, Walter W., B.Sc., 17 Stanhope street. 1886
Blair, G. M'Lellan, 2 Lilybank terrace. 1869
Blair, J. M'Lellan, 2 Bute gardens, Hillhead. 1869
65 Blair, Matthew, 11 Hampton Court terrace. 1887
Blyth, James, M.A., F.R.S.E., Professor of Mathematics and Natural Philosophy, Anderson's College, 204 George street. 1881
*Blyth, Robert, C.A., 1 Montgomerie quadrant. 1885
Borthwick, James D., 8 Broomhill terrace, Partick. 1891
Bost, Wm. David Ashton, Langroods, Paisley. 1884
70 Bost, Timothy, 33 Renfield street. 1876
Bottomley, James T., M.A., D.Sc., F.R.S., F.R.S.E., F.C.S., Demonstrator in Natural Philosophy, University of Glasgow, 13 University gardens, Hillhead. 1880
Bottomley, Wm., C.E., 15 University gardens. 1880
Bower, F. O., D.Sc., M.A., F.L.S., Regius Professor of Botany in the University of Glasgow, 45 Kersland terrace. 1885
Bowie, Campbell T., 26 Bothwell street. 1870
75 Boyd, John, Shettleston Iron-works, near Glasgow. 1873
Boyd, Rev. William, LL.D., 6 Park Circus place. 1885
Brand, James, C.E., 172 Buchanan street. 1880
Brier, Henry, M.I.M.E., Scotch and Irish Oxygen Co., Polmadie. 1889
Brodie, John Ewan, M.D., C.M., F.F.P.S.G., 1 Albany place. 1873
80 Brodie, Maclean, C.A., 44 Westbourne gardens. 1889
Brown, Alexander, 3 Queen's ter. 1887
Brown, G. F. H., 3 South Apsley place. 1889
*Brown, Hugh, 5 St. John's terrace, Hillhead. 1887
Brown, James, 76 St. Vincent st. 1876
85 *Brown, John, 11 Somerset place. 1881
Brown, John C., 149 West George street. 1880
Brown, H. Mathieson, 1 St. James place, Hillhead. 1888
Brown, Richard, Haylee, Largs. 1855
Brown, Robert, 19 Jamaica street. 1882
90 *Brown, Wm. Stevenson, 41 Oswald street. 1886
Brownlie, Archibald, Bank of Scotland, Barrhead. 1880
Brownlie, Jas., 104 Hill street, Garnethill. 1877
Brunton, Rev. Alex., Ardbeg villa, Craigpark, Dennistoun. 1884
*Bryce, Charles C., 141 West George street. 1884
95 Bryce, David, 129 Buchanan street. 1872
*Bryce, Robert, 82 Oswald street. 1886
*Buchan, Wm. P., S.E., 21 Renfrew street. 1875
Buchanan, Alex. M., A.M., M.D., Professor of Anatomy, Anderson's College Medical School, 98 St. George's road. 1876
Buchanan, George S., 85 Candleriggs. 1845
100 *Buchanan, William, 123 Blythswood drive. 1886
Burnet, John, I.A., 167 St. Vincent street. 1850
Burnet, Lindsay, Assoc. M.I.C.E., St. Kilda, Dowanhill. 1882
Burns, J., M.D., 15 Fitzroy place, Sauchiehall street. 1864
Burns, J. Cleland, 1 Park gardens. 1874

105 Callajon, Ventura De, 131 West Regent street. 1886
Cameron, Charles, M.D., LL.D., M.P., 104 Union street. 1870
Cameron, H. C., M.D., 200 Bath st. 1873

Cameron, R., Wellpark, Bathgate. 1873
*Campbell, Sir A. C., Bart., M.P., of Blythswood, Renfrew. 1885
110 *Campbell, J. A., LL.D., M.P., Strathcathro, Brechin. 1848
*Campbell, James, 137 Ingram st. 1885
Campbell, John D., 4 Woodvale place, Copeland road, Govan. 1858
Campbell, John MacNaught, Kelvingrove Museum. 1883
*Campbell, Louis, 3 Eton terrace, Hillhead. 1881
115 Carlile, Thomas, 23 West Nile st. 1851
Carmichael, Neil, M.D., C.M., F. F. P. S. G., Invercarmel, 23 Nithsdale drive, Pollokshields. 1873
Carver, Thomas, A.B., B.Sc., C.E., Oswald hill, Partick. 1890
Cassells, John, 62 Glencairn drive, Pollokshields. 1890
*Cayzer, Charles W., 109 Hope st. 1886
120 Chalmers, James, I.A., 101 St. Vincent street. 1884
Chalmers, P. Macgregor, F.S.A.Scot., 176½ Hope street. 1891
Cherrie, James M., Clutha cottage, Tollcross. 1876
*Chisholm, Samuel, 4 Royal ter., W. 1890
Christie, James, A.M., M.D., F.F.P.S.G., 2 Great Kelvin terrace, Bank street, Hillhead. 1876
125 Christie, John, Turkey-red Works, Alexandria, Dumbartonshire. 1868
Chrystal, W. J., F.I.C., F.C.S., Shawfield Works, Rutherglen. 1882
Church, W. R. M., C.A., 75 St. George's place. 1885
Clapperton, Charles, 16 Lilybank gardens, Hillhead. 1882
Clapperton, John, 9 Crown Circus drive. 1874
130 Clark, Henry E., F.F.P.S., M.R.C.S. Eng., 24 India street. 1876
Clark, John, Ph.D., F.I.C., F.C.S., 138 Bath street. 1870
Clark, John, 9 Wilton crescent. 1872
*Clark, William, 125 Buchanan st. 1876
Clavering, Thos., 27 St. Vincent place. 1856
135 Cleland, A. B. Dick, 15 Newton place. 1871
*Cleland, John, M.D., LL.D., D.Sc., F.R.S., Professor of Anatomy in the University of Glasgow. 1884
Clinkskill, James, 1 Holland place. 1868
*Coats, Joseph, M.D., 31 Lynedoch street. 1873
*Cochran, Robert, 7 Crown circus, Dowanhill. 1877
140 Coghill, Wm. C., 263 Argyle street. 1873
Collins, Sir William, F.R.G.S., 3 Park terrace, East. 1869

Colquhoun, James, 158 St. Vincent street. 1876
Colville, James, M.A., D.Sc., 14 Newton place. 1885
Combe, William, 257 W. Campbell street. 1877
145 Connal, Sir Michael, Virginia buildings. 1848
Connell, Wm., 38 St. Enoch square. 1870
Cooke, Stephen, F.C.S., 85 Buccleuch street. 1886
Copeland, Jas., 7 Dundonald road, Kelvinside. 1869
Copland, Wm. R., M. Inst. C.E., 146 West Regent street. 1876
150 Coste, Jules, French Consulate, 131 West Regent street. 1888
Costigane, John T., Hampton house, Ibrox. 1889
Costigane, William, Clifton house, Pollokshields. 1890
Coubrough, A. Sykes, Blanefield, Strathblane. 1869
Coulson, W. Arthur, 56 George sq. 1888
155 Couper, James, Craigforth house, Stirling. 1862
Cowan, M'Taggart, C.E., 27 Ashton terrace, Hillhead. 1876
Craig, Alexander T., 264 St. Vincent street. 1884
Craig, T. A., C.A., 139 St. Vincent street. 1886
Crawford, David, Ferryfield Printworks, Alexandria, N.B. 1873
160 Crawford, Robert, 84 Miller st. 1886
Crawford, W. B., 104 W. Regent street. 1872
Crawford, Wm. C., M.A., Lockharton gardens, Slateford, Edinburgh. 1869
Cree, Thomas S., 21 Exchange sq. 1869
Crosbie, L. Talbot, Scotstounhill, Whiteinch. 1890
165 Cross, Alexander, 14 Woodlands terrace. 1887
Cruikshank, George M., 62 St. Vincent street. 1885
Cumming, Thos., 14 Montgomerie crescent. 1888
Cunningham, John M., 18 Woodside terrace. 1881
Cunningham, J. R., jun., 30 George square. 1881
170 Curphey, Wm. Salvador, 2 Princes square, Strathbungo. 1883
Cuthbert, Alexander A., 14 Newton terrace. 1885
*Cuthbertson, Sir John N., 29 Bath street. 1850

Dansken, A. B., 179 West George street. 1877

*Dansken, John, I.M., 121 West Regent street. 1876
175 Darling, Geo. E., 178 St. Vincent street. 1870
Darwin, Harry, St. Andrew's Works, 618 Eglinton street. 1891
Davidson, M. Officer, 106 Buchanan street. 1890
Deas, Jas., C.E., 7 Crown gardens, Dowanhill. 1869
Dempster, John, 4 Belmar terrace, Pollokshields. 1875
180 Dennison, William, C.E., 175 Hope street. 1876
Dewar, Duncan, St. Fillans, West Coates, Cambuslang. 1877
*Dick, George Handasyde, 136 Buchanan street. 1887
Dittmar, W., LL.D., F.R.S., F.R.S.E., Professor of Chemistry, Anderson's College, 204 George street. 1875
*Dixon, A. Dow, 10 Montgomerie crescent, Kelvinside. 1873
185 Dobbie, A. B., M.A., University. 1885
Donald, John, Townhead Public School. 1872
Donald, William J. A., 27 St. Vincent place. 1877
Donaldson, James, Gas-works, Cambuslang. 1890
Dougall, Franc Gibb, 167 Canning street. 1875
190 Dougall, John, M.D., C.M., F.F.P.S.G., Professor of Materia Medica, St. Mungo's College, 6 Belmar terrace, Pollokshields. 1876
Douglas, Campbell, I.A., F.R.I.B.A., 266 St. Vincent street. 1870
Downie, Robert, jun., Carntyne Dye-works, Parkhead. 1872
Downie, Thomas, Hydepark Foundry. 1886
Drew, Alex., 175 West George street. 1869
195 Duncan, Eben., M.D., C.M., F.F.P.S.G., 4 Royal crescent, Crosshill. 1873
*Duncan, Robert, Whitefield Works, Govan. 1890
*Duncan, Walter, 9 Montgomerie crescent. 1881
Dunlop, E. D., 40 W. Nile street. 1883
*Dunlop, Nathaniel, 25 Bothwell street. 1870
200 Dunn, Robert Hunter, 4 Belmont crescent. 1878
Dyer, Henry, M.A., D.Sc., C.E., 8 Highburgh terrace, Dowanhill. 1883

Eadie, Alexander, 280 Cathcart road. 1885
Easton, Walter, 125 Buchanan street. 1878
Easton, William J., 150 West Regent street. 1876
205 *Edwards, John, Govanhaugh Dye-works. 1883
Edwards, Matthew, 209 Sauchiehall street. 1887
Elder, James, C.E., 204 St. Vincent street. 1881
Elgar, Francis, LL.D., Admiralty, London. 1884
*Ellis, T. Leonard, North British Iron-works, Coatbridge. 1888
210 Erskine, Jas., M.A., M.B., L.F.P.S., 5 Charing Cross mansions. 1886
*Ewing, Wm., 7 Royal Bank place. 1883

Fairweather, Wallace, C.E., 62 St. Vincent street. 1880
Falconer, Patrick, 33 Hayburn crescent, Partick. 1876
Falconer, Thos., 50 Kelvingrove st. 1880
215 Farquhar, John, 13 Belhaven ter. 1872
Faulds, W. B., Westfield, Ibrox. 1890
Fawsitt, Charles A., 4 Maule terrace, Partick. 1879
Fergus, Freeland, M.B., F.F.P.S.G., 3 Elmbank crescent. 1887
Fergus, Jas., 5 Burnbank gardens. 1880
220 Ferguson, D. Scott, 10 Belhaven terrace. 1890
*Ferguson, John, M.A., LL.D., Professor of Chemistry, University of Glasgow. 1869
Ferguson, Peter, 15 Bute gardens, Hillhead. 1866
Ferguson, Thomas, Westmuir st., Parkhead. 1883
Fergusson, Alex. A., 48 M'Alpine street. 1847
225 Fife, William, 52 Glassford street. 1880
Finlay, H. G., 16 Westbourne terrace. 1888
Finlay, Joseph, Clairmont, Winton drive, Kelvinside. 1873
Finlay, Robert Gilchrist, jun., Holmfield, Dalmuir. 1881
Finlayson, James, M.D., 2 Woodside place. 1873
230 *Fleming, James, 136 Glebe street. 1880
*Fleming, William James, M.D., 3 Woodside terrace. 1876
Fotheringham, T. B., 65 West Regent street. 1889
Foulis, William, C.E., 45 John st. 1870
*Fowler, John, 4 Gray street, Sandyford. 1880
235 Frame, James, Union Bank of Scotland, 113 King street, Tradeston. 1885
Fraser, Matthew P., 91 W. Regent street. 1887

Fraser, Melville, 31 St. Vincent pl. 1890
Fraser, Robert, 2 Crown gardens, Dowanhill. 1885
Frazer, Daniel, 127 Buchanan st. 1853
240 Frew, Alex., C.E., 175 Hope street. 1876
Fullarton, J. H., M.A., B.Sc., Fishery Board Office, Edinburgh. 1886
Fulton, David, Roxburgh villa, Bothwell. 1891
Fulton, R. C., 2 Lugar place, Kelvinside. 1890
Fyfe, Peter, 1 Montrose street. 1886

245 Gairdner, Charles, LL.D., Broom, Newton-Mearns. 1884
*Gairdner, C. D., C.A., 115 St. Vincent street. 1886
Gairdner, W. T., M.D., LL.D., Professor of Practice of Medicine in the University of Glasgow, 225 St. Vincent street. 1863
Galbraith, Peter, 17 Huntly gardens. 1889
Gale, Jas. M., C.E., 45 John street. 1856
250 Galloway, T. Lindsay, C.E., 43 Mair street, Plantation. 1881
Galt, Alex., B.Sc., F.R.S.E., F.C.S., Gowanbrae, Dunoon. 1887
Gardner, Daniel, 36 Jamaica street. 1869
*Garrow, James R., 32 Elmbank cr. 1890
*Garroway, John, 694 Duke st. 1875
255 Geddes, Wm., 8 Battlefield crescent, Langside. 1846
Gillies, W. D., 17 Royal Exchange square. 1872
Gilfillan, Wm., 129 St. Vincent st. 1881
Glaister, John, M.D., F.F.P.S.G., D.P.H., Camb., &c., Professor of Medical Jurisprudence and Public Health, St. Mungo's College, 4 Grafton place. 1879
Goldie, James, 40 St. Enoch square. 1883
260 Goodwin, Robert, 58 Renfield st. 1875
Gorman, C. S., 6 Broomhill avenue, Partick. 1890
Gourlay, John, C.A., 24 George sq. 1874
Gourlay, Robert, Kirklee avenue, Great Western road. 1869
Gow, Leonard, 19 Waterloo street. 1889
265 Gow, Leonard, jun., 19 Waterloo st. 1884
Gow, Robert, Cairndowan, Dowanhill gardens. 1860
Graham, Alex. M., Rowanlea, 7 St. Andrew's drive, Pollokshields. 1887
Graham, David, jun., 140 Douglas street. 1876
Graham, Robert, 108 Eglinton st. 1888
270 *Graham, William, 11 Claremont ter. 1885
Grant, Robt., M.A., LL.D., F.R.S., Professor of Astronomy in the University of Glasgow, Observatory, *Hon. Vice-President.* 1860

Gray, Andrew, 30 Bath street. 1889
Gray, James, M.D., 15 Newton terrace. 1863
Gray, James, 2 Balmoral crescent, Crosshill. 1876
275 Gray, Thomas, B.Sc., F.R.S.E., Rose Institute, Terre Haute, Indiana, U.S.A. 1887
Greenlees, Alex., M.D., 33 Elmbank street. 1864
Grierson, James, 5 Belhaven cres., Kelvinside. 1880
Grieve, John, M.A., M.D., F.R.S.E., care of W. L. Buchanan, 212 St. Vincent st. 1856
Griffiths, Azariah, Elmbank, Falkirk. 1886
280 Guthrie, John, 465 Eglinton street. 1891

Haldane, T. Fred., Cartvale Chemical Works, Paisley. 1884
Halket, George, M.D., F.F.P.S.G., 4 Royal cres., W. 1889
Hamilton, John, I.A., 212 St. Vincent street. 1885
Hannay, Jas. B., F.R.S.E., F.C.S., New Club, West George street. 1879
285 Hart, Arthur, 20 Woodlands terrace. 1883
*Harvie, John, Secretary, Clydesdale Bank, 30 St. Vincent place. 1880
Harvie, William, 8 Bothwell terrace, Hillhead. 1888
*Henderson, A. P., 10 Crown terrace, Dowanhill. 1880
Henderson, George G., D.Sc., M.A., F.I.C., F.C.S., Chemical Laboratory, University. 1883
290 *Henderson, John, jun., Meadowside Works, Partick. 1879
Henderson, John, Towerville, Helensburgh. 1890
Henderson, Robert, 330 Renfrew street. 1885
Henderson, Thos., 47 Union street. 1855
Henderson, Wm., Ennerdale, Winton drive, Kelvinside. 1853
295 *Henderson, Wm., 4 Windsor terrace, West. 1873
Henry, R. W., 8 Belhaven crescent, Kelvinside. 1875
Heys, Zechariah J., South Arthurlie, Barrhead. 1870
Higginbotham, James S., Springfield court, Queen street. 1874
Higginbotham, Robert Ker, 10 Great Hamilton street. 1885
300 Higgins, Henry, jun., 247 St. Vincent street. 1878
Hodge, William, 27 Montgomery drive, Kelvinside. 1878
Hoey, David G., Temple Chambers, London, E.C. 1869

Hogg, Robert, Inglisby villa, Nithsdale drive, Pollokshields. 1865
Holt, T. G., 25 Wellington street. 1875
305 Honeyman, John, F.R.I.B.A., 140 Bath street. 1870
Horne, R. R., C.E., 150 Hope st. 1876
Horton, William, Birchfield, Mount Florida. 1889
Howat, William, 37 Elliot street. 1885
Howatt, James, I.M., 146 Buchanan street. 1870
310 Howatt, William, I. M., 146 Buchanan street. 1870
Hunt, Edmund, 87 St. Vincent st. 1856
*Hunt, John, Milton of Campsie. 1881
*Hunter, Wm. S., 30 Hope street. 1889
Hutchison, Peter, 3 Lilybank terrace, Hillhead. 1889

315 Inglis, R. A., Arden, Bothwell. 1889

*Jack, William, M.A., LL.D., Professor of Mathematics in the University of Glasgow. 1881
Jackson, William V., 25 Stanley street, W. 1888
Jamieson, Andrew, F.R.S.E., M.Inst.C.E., M.Inst.E.E., &c., 38 Bath street. 1881
Jebb, Richard C., M.A., LL.D., Professor of Greek, Cambridge. 1888
320 Johnson, James Yate, C.E., 115 St. Vincent street. 1883
Johnston, David, 100 West George street. 1891
Johnstone, Jas., Coatbridge street, Port-Dundas. 1869
Jolly, William, F.G.S., F.R.S.E., Greenhead house, Govan. 1890

Kay, Wm. E., F.C.S., Gowanbank, Clarkston, Busby. 1887
325 Kean, James, 32 Scotia street, Garnethill. 1888
Kelly, James K., M.D., F.F.P.S.G., Park villa, Queen Mary avenue, Crosshill. 1889
Kemp, Ebenezer, Overbridge, Ibrox. 1889
Kennedy, Hugh, Redclyffe, Partick. 1876
Kennedy, James, 33 Greendyke st. 1889
330 Ker, Charles, M.A., C.A., 115 St. Vincent street. 1885
*Ker, Wm., 1 Windsor ter., west. 1874
Keir, Adam, 175 Trongate. 1887
Kerr, Charles James, 40 West Nile street. 1877
Kerr, Geo. Munro, 97 Buchanan st. 1890
335 Kerr, James Hy., 13 Virginia st. 1872
Kerr, John G., M.A., 15 India st. 1878
Key, William, Tradeston Gas-works. 1877
King, James, 57 Hamilton drive, Hillhead. 1848
King, Sir James, Bart., LL.D., of Campsie, 115 Wellington street. 1855
340 Kirk, Alexander C., LL.D., 19 Athole gardens, Dowanhill. 1869
Kirk, Robert, M.D., Newton cottage, Partick. 1877
Kirkpatrick, Alexander B., 24 Berkeley terrace. 1885
Kirkpatrick, Andrew J., 179 West George street. 1869
Kirkwood, James, Carling lodge, Ibrox. 1890
345 Knox, Adam, 47 Crownpoint road. 1881
*Knox, David J., 129 West George street. 1890
Knox, John, 58 Bath street. 1883

Laird, George H., 159 Greenhead street. 1882
Laird, John, Marchmont, Port-Glasgow. 1876
350 Laird, John, Royal Exchange Sale Rooms. 1879
Lamb, Thomas, 220 Parliamentary road. 1870
Lang, William, jun., F.C.S., Crosspark, Partick. 1865
Latta, James, 73 Mitchell street. 1869
Latta, John, 138 West George st. 1880
355 Lazenby, Rev. Albert, 50 Prince's square, Strathbungo. 1885
Leggat, William, Buchanan Institution. 1889
Leitch, Alexander, 60 Rosebank terrace, Grant street. 1886
Lester, William, 2 Doune terrace, N. Woodside. 1884
Lester, W. R., M.A., 2 Doune terrace, N. Woodside. 1884
360 *Lindsay, Archd. M., M.A., 87 West Regent street. 1872
Lindsay, Wm. G., 156 St. Vincent street. 1871
Lochore, John, 8 Bellahouston ter., Ibrox. 1886
*Long, John Jex, 11 Doune terrace, Kelvinside. 1862
Lothian, J. Alexander, M.D., L.R.C.S.E., 6 Newton terrace. 1872
365 Love, James Kerr, M.D., C.M., 4 Matilda place, Strathbungo. 1888
Lundholm, C. O., Nobel's Explosives Factory, Ardeer, Stevenston. 1890

M'Andrew, John, 17 Park Circus place. 1843
M'Ara, Alex., 65 Morrison street. 1888
Macarthur, J. G., Rosemary villa, Bowling. 1874
370 Macarthur, John S., 13 West Scotland street. 1890

M'Call, Samuel, 16 Hillsborough square, Hillhead. 1882
M'Callum, Robert, jun., 69 Union street. 1891
*M'Clelland, Andrew Simpson, C.A., 4 Crown gardens, Dowanhill. 1884
M'Conville, John, M.D., 27 Newton place. 1870
375 M'Cracken, James, 5 Bowmont terrace, Kelvinside. 1889
M'Crae, John, 7 Kirklee gardens, Maryhill. 1876
M'Creath, James, M.E., 208 St. Vincent street. 1874
Macdonald, Arch. G., 8 Park circus. 1869
Macdonald, Thomas, 205 St. Vincent street. 1869
380 Macdonald, Thomas F., M.B., C.M., Burgh house, Maryhill. 1889
M'Farlane, Graham Jas., Elderslie. 1882
Macfarlane, Walter, 12 Lynedoch crescent. 1885
M'Farlane, Wm., Edina lodge, Rutherglen. 1888
M'Gillivray, James P., 207 West Campbell street. 1883
385*M'Gilvray, R. A., 129 West Regent street. 1880
M'Gregor, Duncan, F.R.G.S., 37 Clyde place. 1867
M'Gregor, James, 1 East India avenue, London, E.C. 1872
M'Houl, David, Ph.D., Dalquhorn Works, Renton. 1883
*M'Ilwraith, James, 4 Westbourne terrace, Kelvinside. 1872
390 M'Intyre, Wm., Marion bank, Rutherglen. 1888
M'Ivor, R. W. Emerson, F.I.C., F.C.S., St. George's Club, Hanover square, London. 1886
Mackay, John Yule, M.D., 34 Elmbank crescent. 1885
Mackay, John, jun., 354 Sauchiehall street. 1869
*M'Kenzie, W. D., 43 Howard st. 1875
395*M'Kenzie, W. J., 86 Mitchell st. 1879
*M'Kendrick, John G., M.D., C.M., LL.D., F.R.S., F.R.S.E., F.R.C.P.E., Professor of Institutes of Medicine in the University of Glasgow, 45 Westbourne gardens, *President.* 1877
Mackinlay, David, 6 Great Western terrace, Hillhead. 1855
*Mackinlay, James Murray, 4 Westbourne gardens. 1886
Mackinlay, Wm., 4 Bothwell terrace, Hillhead. 1887
400 M'Kissack, John, 68 W. Regent st. 1881
Maclae, A. Crum, 147 St. Vincent street. 1884
MacLean, Walter, 2 Bothwell cir. 1887
*Maclay, David T., 169 W. George street. 1879
Maclean, A. H., 8 Hughenden terrace, Kelvinside. 1870
405 Maclean, Magnus, M.A., F.R.S.E., 21 Hayburn crescent, Partickhill. 1885
MacLehose, James J., M.A., 61 St. Vincent street. 1882
M'Lennan, James, 40 St. Andrew's street. 1888
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Macphail, Donald, M.D., Garturk cottage, Whifflet, Coatbridge. 1877
410 M'Phee, Donald, 4 Kirklee road, Kelvinside. 1889
M'Pherson, George L., 26 Albert road, Crosshill, East. 1872
M'Vail, D. C., M.B., 3 St. James' terrace, Hillhead. 1873
M'Whirter, William, Faraday Electrical Works, Govan. 1889
Machell, Thomas, 39 Great Western road. 1886
415 Main, Robert B., Milverton, Dalziel drive, Pollokshields. 1885
Mann, John, C.A., 188 St. Vincent street, *Treasurer.* 1856
Mann, John, jun., M.A., C.A., 188 St. Vincent street. 1885
Manwell, James, The Hut, 4 Albert drive, Pollokshields. 1876
Martin, W. C., 137 West Regent st. 1889
420 Marwick, Sir J. D., LL.D., F.R.S.E., Killermont house, Maryhill. 1878
Marks, Samuel, Jeanette villa, Tollcross. 1884
Mathieson, Thomas A., 3 Grosvenor terrace. 1869
Mavor, Alfred E., 4 Elmbank cres. 1890
Mavor, Henry A., 56 George square. 1887
425 Mavor, James, 63 Bank street, Hillhead. 1885
Mavor, Samuel, 4 Elmbank cres. 1890
Mayer, John, 2 Royal crescent, Crosshill, *Secretary.* 1860
Mechan, Arthur, 60 Elliot street. 1876
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430 Mees, A. R., 136 W. Regent street. 1888
Meikle, Andrew W., M.A., Viewfield house, Pollokshields. 1890
Menzies, Thos., Hutchesons' Grammar School, Crown street. 1859
*Menzies, Thos. J., M.A., B.Sc., F.C.S., Stranraer Academy, Stranraer. 1887
Michaelson, M., 21 Huntly gardens. 1878
435 Middleton, Robert T., 179 West George street. 1860
Millar, James, 158 Parliamentary road. 1870

Miller, A. Lindsay, 121 W. Regent street. 1878
*Miller, Arch. Russell, The Cairns, Cambuslang. 1884
Miller, David S., 8 Royal crescent, W. 1887
440*Miller, George, Winton drive, Kelvinside. 1881
Miller, G. J., Frankfield, Shettleston. 1888
Miller, John (Messrs. James Black & Co.), 23 Royal Exchange square. 1874
Miller, Richard, 54 St. Enoch sq. 1885
*Miller, Thos. P., Cambuslang Dyeworks. 1864
445 Miller, W. M., 7 Mansfield place, West Regent street. 1867
Mills, Edmund J., D.Sc., F.R.S., "Young" Professor of Technical Chemistry, 60 John street. 1875
Milne, William, M.A., B.Sc., F.R.S.E., High School. 1881
Mirrlees, James B., Redlands, Kelvinside. 1866
*Mirrlees, William J., 42 Aytoun road, Pollokshields. 1889
450 Mitchell, George A., 67 West Nile street. 1883
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*Moffatt, Alexander, 23 Abercromby place, Edinburgh. 1874
Moir, Charles S., 92 Union street. 1884
455 Mollison, James, 6 Hillside gardens, Partick. 1889
*Mond, Robert Ludwig, B.A. (Cantab), F.R.S.E., Physical Laboratory, University. 1890
*Monteith, Robert, Greenbank, Dowanhill gardens. 1885
Moore, Alexander, C.A., 209 West George street. 1869
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460 Morgan, John, Springfield house, Bishopbriggs. 1844
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Motion, James Russell, 38 Cochrane street. 1887
Muir, Alex., 400 Eglinton street. 1883
*Muir, Allan, 36 George street. 1881
465 Muir, James, C.A., 149 West George street. 1887
Muir, John, 6 Park gardens. 1876
Muir, Thomas, M.A., LL.D., F.R.S.E., Beechcroft, Bothwell. 1874
*Muirhead, Andrew Erskine, Cart Forge, Crossmyloof. 1873
Muirhead, James, 10 Doune gardens, Kelvinside. 1887
470*Muirhead, Robert F., M.A., B.Sc., Mason College, Birmingham. 1879
Munro, Daniel, 10 Doune terrace, Kelvinside. 1867
Munsie, George, 1 St. John's ter., Hillhead. 1871
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*Murdoch, Robert, 19 Commerce st. 1880
475 Murdoch, William, 79 Robertson street. 1879
*Murray, David, LL.D., 169 West George street. 1876
Murray, John Bruce, 65 Great Clyde street. 1890
Murray, A. Erskine, Sheriff-Substitute of Lanarkshire, Sundown, Montgomerie drive. 1881

Napier, Alex., M.D., F.F.P.S.G., Rose Bank, Queen Mary avenue, Crosshill. 1886
480 Napier, James, 15 Prince's square, Strathbungo. 1870
Napier, James A., 55 Cathedral st. 1890
*Napier, John, 23 Portman square, London. 1846
Nelson, Alex., 80 Gordon street. 1880
Nelson, D. M., 164 St. Vincent street. 1875
485*Newlands, Joseph F., 28 Renfield street. 1883
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490 Orr, Robert, 79 West Nile street. 1890
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495*Parker, John Dunlop, C.E., 146 West Regent street. 1889
*Parnie, James, 32 Lynedoch street. 1874
Paterson, John, 522 Pollokshaws road. 1888
*Paterson, Robert, C.A., 28 Renfield street. 1881
Paton, James, F.L.S., Corporation Galleries, and Kelvingrove Museum. 1876

500 Patterson, T. L., F.C.S., at John Walker & Co.'s, Greenock. 1873
Petrie, Alexander, I.A., 109 Bath street. 1885
Pirie, John, M.D., 26 Elmbank crescent. 1877
*Pirrie, Robert, 9 Buckingham ter. 1875
*Pollock, R., M.B., C.M., F.F.P.& S.G., Laurieston house, Pollokshields. 1883
505 Price, Rees, L.D.S., Eng., 163 Bath street. 1883
Pride, David, M.D., Townhead House, Neilston. 1887
*Provan, James, 40 West Nile st. 1868
Provand, A. D., M.P., 8 Bridge street, London, S.W. 1888

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510 Ramsay, John, of Kildalton, 5 Dixon street. 1856
Ramsay, Robert, M.D., L.R.C.S.E., Lochwinnoch. 1881
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Rankine, David, C.E., 5 West Regent street. 1875
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515 Rattray, Rev. Alex., M.A., Parkhead parish, 4 Westercraigs, Dennistoun. 1879
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Reid, David, 16 Cambridge street. 1887
*Reid, Hugh, Belmont, Springburn. 1880
520 Reid, James, 10 Woodside terrace. 1870
Reid, James, 15 Montgomerie cres. 1889
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525 *Reid, William L., M.D., 7 Royal crescent, West. 1882
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Rey, Hector, B.L., B.Sc., 2 Vinicombe street, Hillhead. 1889
Richmond, Thos., L.R.C.P.E., 26 Burnbank terrace. 1887
530 Ritchie, George, Parkhead Forge and Steel Works. 1890
Robertson, Archibald, 25 Queen st. 1863
Robertson, Rev. James, D.D., Professor of Oriental Languages in the University of Glasgow. 1884
Robertson, John, 10 Valeview ter., Langside, *Librarian.* 1860
Robertson, J. M'Gregor, M.A., M.B., 400 Great Western road. 1881
535 Robertson, Robert, Coplawhill, Pollokshaws road. 1877
Robertson, Robert A., 8 Park street, East. 1877
Robertson, Robert H., Clyde bank, Rutherglen. 1888
Robertson, William, C.E., 123 St. Vincent street. 1869
*Rogers, John C., 224 St. Vincent st. 1888
540 Rose, Alexander, Richmond house, Dowanhill. 1879
*Rose, Charles A., 1 Belhaven cres. 1889
Ross, David, M.A., B.Sc., LL.D., E.C. Training College. 1888
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*Ross, John, 9 Westbourne gardens. 1885
545 Rottenburg, Paul, 21 St. Vincent place. 1872
Rowan, David, 22 Woodside place. 1863
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Rundell, R. Cooper, Underwriters' Room, Royal Exchange. 1877
Russell, James B., B.A., M.D., LL.D., 3 Foremount terrace, Partick, *Hon. Vice-President.* 1862

550 Salmon, W. Forrest, F.R.I.B.A., 197 St. Vincent street. 1870
Sayers, William Brooks, 56 George square. 1890
Schmidt, Alfred, 492 New City road. 1881
Scott, Alex., 2 Lawrence place, Dowanhill. 1871
*Scott, D. M'Laren, 2 Park quadrant. 1881
555 Scott, John, 140 Douglas street. 1891
Scott, Robt., I.M., 115 Wellington street. 1884
Seligmann, Hermann L., 135 Buchanan street. 1850
Shields, Thomas, M.A., 12 Queen's crescent, Cathcart. 1890
Simons, Michael, 206 Bath street. 1880
560 Simpson, P. A., M.A. Cantab., M.D., Regius Professor of Forensic Medicine, University, 1 Blythswood square. 1881
Sinclair, Alexander, Ajmere lodge, Langside. 1883
Smart, William, M.A., Nunholm, Dowanhill. 1886
Smellie, George, I.M., 167 St. Vincent street. 1880
*Smellie, Thos. D., 209 St. Vincent street. 1871
565 Smith, D. Johnstone, C.A., 149 W. George street. 1888
Smith, Francis, 45 Gordon street. 1875
Smith, Harry J., Ph.D., Coltness Iron-works, Newmains. 1877

Smith, Hugh C., 55 Bath street. 1861
*Smith, J. Guthrie, 54 West Nile st. 1875
570*Smith, Robert B., Bonnybridge, Stirlingshire. 1884
Smith, W. R. W., Rosmor, Sandbank. 1868
Smith, William, jun., 1 University Gardens terrace, Hillhead. 1890
Snodgrass, William, M.A., M.B., C.M., Muirhead Demonstrator of Physiology, University of Glasgow. 1890
*Somerville, Alexander, B.Sc., F.L.S., 4 Bute Mansions, Hillhead street, Hillhead. 1888
575 Somerville, David, 35 King st, S.S. 1885
Sorley, Robert, 3 Buchanan st. 1878
Spencer, Charles L., Edgehill, Kelvinside. 1891
Spens, John A., 169 W. George st. 1879
*Spiers, John, 43 Great Western road, Hillhead. 1885
580 Stanford, Edward C. C., F.C.S., Glenwood, Dalmuir, Dumbartonshire. 1864
*Steel, William Strang, Braco Castle, Braco, Perthshire. 1889
*Stephen, John, Domira, Partick. 1880
Stephen, Robt. R., Adelphi Biscuit Factory. 1867
*Steven, Hugh, Westmount, Montgomerie drive. 1869
585 Steven, John, 32 Elliot street. 1875
*Stevenson, D. M., 12 Waterloo st. 1889
*Stevenson, Jas., F.R.G.S., 23 West Nile street. 1870
Stevenson, John, C.E., 208 St. Vincent street. 1885
Stevenson, William, Tower bank, Lenzie. 1870
590 Stevenson, Wm., 21 Clyde place. 1888
Stewart, Andrew, 41 Oswald street. 1887
Stewart, David, 3 Clifton place. 1856
Stewart, James Reid, 30 Oswald street. 1845
Stewart, John, Western Saw Mills. 1877
595 Stobo, Thomas, Somerset house, Garelochhead. 1884
Stoddart, James Edward, Howden, Mid-Calder, N.B. 1872
*Strain, John, C.E., 154 West George street. 1876
Summers, John, 174 West Nile st. 1891
*Sutherland, David, Great Western Hotel, Oban. 1880
600*Sutherland, John, Great Western Hotel, Oban. 1880
Sutherland, J. R., 6 Clifton place. 1884
Sutherland, Malcolm, Leven Shipyard, Dumbarton. 1890
Sutherland, Robert, 28 Woodside place. 1875
Sutherland, Thos., 198 Parliamentary road. 1886
605 Swan, Charles C., 15 Rose street, Garnethill. 1891

Tatlock, John, F.I.C., 34 Gray street, Sandyford. 1875
Tatlock, Robt. R., F.R.S.E., F.I.C., F.C.S., 156 Bath street. 1868
Taylor, Benjamin, F.R.G.S., 10 Derby crescent, Kelvinside. 1872
Taylor, Thomas, 60 Montrose street. 1889
610 Teacher, Adam, 14 St. Enoch sq. 1868
Tennant, Sir Charles, Bart., 195 West George street. 1868
Tennant, Gavin P., M.D., 159 Bath street. 1875
Thomas, Moses, M.D., Superintendent, Royal Infirmary. 1890
Thomson, David, I.A., F.R.I.B.A., 2 West Regent street. 1869
615 Thomson, George C., F.C.S., 86 St. Vincent street. 1883
Thomson, Gilbert, M. A., C. E., 75 Bath street. 1885
Thomson, Graham Hardie, 2 Marlborough terrace, Kelvinside. 1869
*Thomson, James, F.R.I.B.A., 88 Bath street. 1886
Thomson, James, F.G.S., 26 Leven street, Pollokshields. 1863
620 Thomson, Jas., LL.D., F.R.S., C.E., 2 Florentine gardens, Hillhead. 1874
Thomson, Jonathan, 3 St. John's terrace, Hillhead. 1869
Thomson, Sir William, LL.D., D.C.L., F.R.SS., L. & E., Professor of Natural Philosophy, University of Glasgow, *Hon. Vice-President.* 1846
Thomson, William, B.A., 32 Granby terrace. 1888
Townsend, C. W., Crawford street, Port-Dundas. 1890
625*Tullis, James Thomson, Anchorage, Burnside, Rutherglen. 1883
Turnbull, John, 37 West George st. 1843
*Turnbull, John, jun., M.I.M.E., 255 Bath street. 1883
Turner, George A., M.D., 1 Clifton place, Sauchiehall street. 1883
Turner, William, Rachan house, Helensburgh, N.B. 1875

630 Verel, Wm. A., The Linn, Cathcart. 1883

Walker, Adam, 35 Elmbank cres. 1880
*Walker, Archibald, B.A. (Oxon.), F.C.S., 8 Crown ter., Dowanhill. 1885
Walker, James A., 112 St. Vincent street. 1884

Walker, Malcolm M'N., F.R.A.S., 7 Westbourne ter., Fort Matilda, Greenock. 1853
635*Wallace, Hugh, 30 Havelock street. 1879
*Wallace, Wm., M.A., M.B., C.M., Westfield house, Shawlands. 1888
Wallace, William, M.A., Allan Glen's School. 1890
Wardlaw, Johnston, 83 Taylor street. 1884
Warren, John A., C.E., 115 Wellington street. 1887
640 Watson, Archibald, 5 Westbourne terrace. 1881
Watson, James, 24 Sandyford place. 1873
Watson, John, 205 West George street. 1886
Watson, Joseph, 225 West George street. 1882
Watson, J. Robertson, M.A., Professor of Chemistry, Anderson's College Medical School. 1891
645*Watson, Thomas Lennox, I.A., F.R.I.B.A., 108 W. Regent st. 1876
*Watson, William Renny, 16 Woodlands terrace. 1870
Weir, Walter, C.A., Barskiven, Paisley. 1888
Welsh, Thomas M., 3 Prince's gardens, Dowanhill. 1883
Wenley, James A., Bank of Scotland, Edinburgh. 1870
650 Westlands, Robert, 99 Mitchell street. 1869
White, John, Scotstoun Mills, Partick. 1875
*Whitson, Jas., M.D., F.F.P.& S.G., 13 Somerset place. 1882
Whytlaw, R. A., 1 Windsor quadrant, Kelvinside. 1885
Widmer, Justus, 21 Athole gardens. 1887
655 Williamson, John, 65 West Regent street. 1881
Wilson, Alex., Hydepark Foundry 54 Finnieston street. 1874
Wilson, Charles, 20 Lynedoch street. 1875
Wilson, David, Carbeth, by Killearn. 1850
Wilson, Richard J., St. George's Road Public School. 1887
660 Wilson, William, Virginia buildings. 1881
Wilson, William, 290 Renfrew st. 1889
Wilson, W. H., 45 Hope street. 1881
Wingate, Arthur, 6 Kelvin drive. 1882
*Wingate, John B., 7 Crown terrace, Dowanhill. 1881
665 Wingate, P., 14 Westbourne ter. 1872
Wingate, Walter E., 4 Bowmont ter. 1880
Wood, James, M.A., Glasgow Academy. 1885
Wood, James, 28 Royal Exchange square. 1886
Wood, Wm. Copland, Turkey-red Works, Alexandria. 1883
670 Wood, W. E. H., 40 Candleriggs. 1891
Woodburn, J. Cowan, M.D., 197 Bath street. 1869
Wright, Alexander Leckie, Coats Iron and Steel Works, Coatbridge. 1890
Wyper, James, 6 Burnbank gardens. 1878

Yellowlees, D., M.D., LL.D., Physician-Superintendent, Gartnavel. 1881
675 Young, George Christie, City Saw Mills, Port-Dundas. 1884
Young, John, 22 Belhaven terrace, Kelvinside. 1885
Young, John, 64 Cochrane street. 1881
*Young, John, jun., M.A., B.Sc., 38 Bath street. 1887
Young, R. Bruce, M.A., M.B., C.M., University. 1885
680*Young, Thos. Graham, Westfield, West Calder. 1880
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